

SCIENCE.

FRIDAY, SEPTEMBER 5, 1884.

THE AMERICAN ASSOCIATION MEETING IN PHILADELPHIA.

IN selecting Philadelphia as the place of its meeting in 1884, the American association for the advancement of science has returned to its birthplace. Forty-four years ago, a score of geologists—one of whom, James Hall, still lives in honor and vigor—assembled in the rooms of the academy of sciences, and formed the Association of geologists and naturalists. Three years later, in Boston, it was decided to enlarge this association; and so at another meeting in Philadelphia, on the 20th of September, 1848, under the guidance of a leader who has but lately departed,—the beloved William B. Rogers,—a new constitution was adopted, other scientific professors and workers were enlisted, and the association, as we now know it in its catholic comprehensiveness, was launched upon its course.

The new name and the new form were doubtless suggested by the British association, which dates its origin from the meeting at York in 1831. The object of the American society was declared to be, “by periodical and migratory meetings to promote intercourse between those who are cultivating science in different parts of the United States; to give a stronger and more general impulse and a more systematic direction to scientific research in our country; and to procure for the labors of scientific men increased facilities and a wider usefulness.” Subsequently the scope of the society was extended, and ‘America’ took the place of ‘United States.’

In again re-assembling at Philadelphia, when time has reversed the digits from ’48 to ’84, it is natural to consider how far the purposes of the founders have been fulfilled. The most sanguine among them could not have anticipated such a growth of scientific endowments, in the period of one generation, as we now look

back upon. The museums, the laboratories, the surveys, the observatories, the professorships, the schools of science and technology, which have been established and maintained since the association was formed, not only in the east but in the west, are results which, perhaps, may not be directly attributed to the association, but which certainly would never have been secured without a wide diffusion of scientific enthusiasm, such as usually follows these migratory congresses of investigators and teachers. We are among those who believe, that, if it could be shown that the association does nothing for the intellectual advantage of its members, nevertheless, all the efforts expended in its management are rewarded like the labors of the sowers in the springtime. The seed springs up, ‘some ten, some sixty, some a hundred fold.’ The educated people in every community, whether they are specially interested in science or not, are always attracted to the meetings; and the reports of papers and addresses are read far and wide through the land. Impressions are thus made in respect to the importance of different lines of research; and the names of scientific leaders become known to those who would never enter the dens and caves of the specialists, and would never have the benefit of their inspiration were it not for these autumnal conferences.

Rarely is there a meeting of the association which does not afford striking examples of the relations of government to science, and of the importance of securing for the public the results of prolonged research. Astronomy, geology, geodesy, certain branches of physics, ethnology, and now biology, through the admirable studies of the U. S. fish-commission, receive their most generous encouragement from the national government. To make the same assertion in another form, we may say that an enlightened people insists upon it that congress shall secure, for the good of all citizens, whatever results can be obtained by the liberal employment of

science in the public service. More than this, individual citizens have discovered that there is no better use for wealth than by endowments like those which are annually added to the educational resources of the country. In aiding all such tendencies, the American association has performed a noble part.

As we have seen, the founders of the association declared as their first object the promotion of "intercourse between those who are cultivating science in different parts of the United States." One of the obstacles to progress in this country is the wide separation of those who are workers in kindred departments. A professor in Dublin or in Edinburgh may go to London in a night; but it takes seven days for our California friends, and half that time for many a professor in the interior, to reach Washington or Boston. So much the more reason is there that these annual congresses, bringing people together from every part of the land, should be kept up. Acquaintances, friendships, copartnerships, promotions, criticisms, suggestions, assistance, are the fruits of this intercourse. Those who live in the centre of scientific activities, who see more people of mark in every month than are to be seen at other places in a year, are in danger of undervaluing all popular assemblies and conventions, and are tempted to stay away from the unsatisfactory throng. But it has been fortunate that nearly all the most eminent members of the American association have been ready to attend these meetings frequently, if not invariably, and to give the encouragement of their presence, their counsel, and their friendly greetings, to those who were younger. Not to mention any who are living, was there ever a more benignant and inspiring teacher than Agassiz? did any one ever forget the greetings of Bache who once felt his friendly grasp? and could anybody be more ready than Henry to lend a helpful hand to all who needed encouragement? Are there not scores of workers in the field to-day who remember with gratitude this trio, and others of their kin, as they appeared, for instance, at the Albany meeting when the association was in the first flush of

its youthful vigor? Are there not like recollections of the great assembly of 1880, when Boston and Cambridge gave such admirable facilities for seeing institutions and men?

It seems to us that there is always danger of so multiplying the number of meetings, and of so subdividing the sections, as to confuse the members of the association, detract from the general interest, and interfere with the exchange of personal courtesies. The remedy lies with the officers of the association, preventing with firm and judicious decisions the reading of poor papers, and cutting off the discussions of wordy and rambling speakers. A few able papers are much better worth the consideration of the association than a multitude of unimportant communications. *Ponderanda non numeranda.*

As we write these lines, the meeting has not begun; but the circulars which have been issued show that every thing has been done in Philadelphia which experience in hospitality can suggest for the pleasure of the association. We trust that the reflex influences of the gathering will be felt upon the new institute of biology, on the great schools of medicine, on the University of Pennsylvania, on the Academy of natural sciences, and on all the other scientific foundations of which the city is justly proud. The seat of the American philosophical society is a shrine which the countrymen of Franklin and Rittenhouse will visit with pleasure under the presidency of Lesley.

J. PETER LESLEY.

THE subject of this notice was born Sept. 17, 1819, in Philadelphia. Both his grandfather and father were cabinet-makers, intelligent, strong, and honest men, who brought up large families in the faith of the Church of Scotland, and in a love for hard work of every kind, physical and intellectual. He was sent to school on his sixth birthday, to the academy on his twelfth, and to the University of Pennsylvania on his fifteenth, getting his diploma in 1838. At an early age, his religious experiences were of the severest type. He knew

the Bible, especially the Old Testament, almost by heart; had read Bunyan's 'Pilgrim's progress' scores of times, and 'Robinson Crusoe' still oftener; went three times to church every Sunday, and twice to Sabbath school; and was in the best trim for becoming a candidate for the ministry, to which, in fact, he had been secretly dedicated from his birth. The family physician, however, refused to permit him to enter Princeton seminary after graduating from college; and an accident threw into his lap a commission as sub-assistant on the Geological survey of Pennsylvania, under the direction of Prof. Henry D. Rogers, so that it was only in the autumn of 1841 that he commenced his theological studies at Princeton, N.J.

He early announced his intention not to apply for ordination, but to spend his ministry among the castaway people of the Alleghany Mountains, with whose wretched spiritual condition he had become acquainted in the course of his geological surveys. To give himself a

closer and deeper view into the character of the descendants of the original immigrants to Pennsylvania, he sailed from New York to Liverpool as a steerage passenger in 1844, walked through England and around France with a knapsack and blouse, visited the Waldenses of Dauphine

and the Pietists in Geneva, crossed the Jura on foot, and middle Germany, and spent the winter months with Tholuck at the university at Halle, returning in a sailing-vessel from Bremen to Philadelphia the following May. He spent the next two years in the mountain-districts of the state, until his naturally vigorous constitution gave way, under incessant mental excitement, bodily fatigue and exposure.

A long illness ensued; but late in 1848 the worn-out missionary became the salaried pastor of an Orthodox-Congregationalist church near Boston, and continued in that capacity three years, when, bidding farewell to his parish and to theology, he returned to his native place and natural science, to commence life anew.



J. P. Lecky,

The next ten years were the most active of his life, most of the time being spent in the field. In 1856 he published a "Manual of coal and its topography, illustrated by original drawings, chiefly of facts in the geology of the Appalachian region of the United States," and was appointed secretary of the American iron association. In 1859 appeared his "Iron manufacturer's guide to furnaces, forges, and rolling-mills of the United States, with discussions of iron as a chemical element, an American ore, and a manufactured article, in commerce and in history." Jan. 15, 1858, while examining the iron-works of southern Ohio, Mr. Lesley was elected librarian, and Jan. 7, 1859, one of the four secretaries of the American philosophical society, and continues to hold these offices, which, for the first year or two, withdrew him almost entirely from field-work. From 1860 to 1866 Mr. Lesley was busily employed by capitalists to pronounce upon projected mining-plants, and by mine-owners to examine their properties, the call for iron and coal being great on account of the civil war. In 1864 and 1865 serious illnesses, produced by overwork, prepared the way for a complete breakdown of his nervous system in the early summer of 1866, from which he did not recover until the early winter of 1869, this interval of three years and a half being spent mostly in Europe.

Mr. Lesley went from Italy to the Paris exposition of 1867 as one of the ten commis-

sioners appointed by the United States senate; but his illness steadily increased, and he was compelled to abandon his duties. Not until 1872 could he again do six hours of hard work a day; and a new career of usefulness was opened to him by his appointment, in that year, to the professorship of geology in the new department of science of the University of Pennsylvania, and in 1873 to the directorship of the Second geological survey of the state, still in progress. For four years (1873-78) he performed the duties of both offices, finding his only relaxation in a short voyage to Europe every two years; but a threatening recurrence of his former malady induced him to offer his resignation to the trustees of the university, who, however, preferred to grant him an indefinite furlough, until the close of the geological survey.

With what untiring zeal he has devoted himself to the work of that survey is only known to those who have been associated with him in the work. How successfully he has conducted it, is shown to the world through the seventy volumes recording its progress. If his hundreds of papers, scientific and literary, read before the American philosophical society, had never been published, this great work alone would place him in the front rank of American geologists. Of the personal character of a man whose modesty is his most prominent trait, it is difficult to speak as one would wish during his life.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

PENDING PROBLEMS OF ASTRONOMY.¹

THIRTY-SIX years ago this very month, in this city, and near the place where we are now assembled, the American association for the advancement of science was organized, and held its first meeting. Now, for the first time, it revisits its honored birthplace.

Few of those present this evening were, I suppose, in attendance upon that first meeting. Here and

there, among the members of the association, I see, indeed, the venerable faces of one and another, who, at that time in the flush and vigor of early manhood, participated in its proceedings and discussions; and there are others, who, as boys or youths, looked on in silence, and listening to the words of Agassiz and Peirce, of Bache and Henry, and the Rogers brothers and their associates, drank in that inspiring love of truth and science which ever since has guided and impelled their lives. Probably enough, too, there may be among our hosts in the audience a few who remember that occasion, and were present as spectators.

¹ Address to the American association for the advancement of science at Philadelphia, Sept. 5, 1884, by Prof. C. A. YOUNG, professor of astronomy at Princeton, retiring president of the association.

But, substantially, we who meet here to-day are a new generation, more numerous certainly, and in some respects unquestionably better equipped for our work, than our predecessors were; though we might not care to challenge comparisons as regards native ability, or clearness of insight, or lofty purpose.

And the face of science has greatly changed in the mean time; as much, perhaps, as this great city and the nation. One might almost say, that, since 1848, 'all things have become new' in the scientific world. There is a new mathematics and a new astronomy, a new chemistry and a new electricity, a new geology and a new biology. Great voices have spoken, and have transformed the world of thought and research as much as the material products of science have altered the aspects of external life. The telegraph and dynamo-machine have not more changed the conditions of business and industry than the speculations of Darwin and Helmholtz and their compeers have affected those of philosophy and science.

But, although this return to our birthplace suggests retrospections and comparisons which might profitably occupy our attention for even a much longer time than this evening's session, I prefer, on the whole, to take a different course; looking forwards rather than backwards, and confining myself mainly to topics which lie along the pathway of my own line of work.

The voyager upon the inland sea of Japan sees continually rising before him new islands and mountains of that fairyland. Some come out suddenly from behind nearer rocks or islets, which long concealed the greater things beyond; and some are veiled in clouds which give no hint of what they hide, until a breeze rolls back the curtain; some, and the greatest of them all, are first seen as the minutest specks upon the horizon, and grow slowly to their final grandeur. Even before they reach the horizon line, while yet invisible, they sometimes intimate their presence by signs in sky and air; so slight, indeed, that only the practised eye of the skilful sailor can detect them, though quite obvious to him.

Somewhat so, as we look forward into the future of a science, we see new problems and great subjects presenting themselves. Some are imminent and in the way, — they must be dealt with at once, before further progress can be made; others are more remotely interesting in various degrees; and some, as yet, are mere suggestions, almost too misty and indefinite for steady contemplation.

With your permission, I propose this evening to consider some of the pending problems of astronomy, — those which seem to be most pressing, and most urgently require solution as a condition of advance; and those which appear in themselves most interesting, or likely to be fruitful, from a philosophic point of view.

Taking first those that lie nearest, we have the questions which relate to the dimensions and figure of the earth, the uniformity of its diurnal rotation, and the constancy of its poles and axis.

I think the impression prevails, that we already

know the earth's dimensions with an accuracy even greater than that required by any *astronomical* demands. I certainly had that impression myself not long ago, and was a little startled on being told by the superintendent of our Nautical almanac that the remaining uncertainty was still sufficient to produce serious embarrassment in the reduction and comparison of certain lunar observations. The length of the line joining, say, the Naval observatory at Washington with the Royal observatory at the Cape of Good Hope, is doubtful; not to the extent of only a few hundred feet, as commonly supposed, but the uncertainty amounts to some thousands of feet, and may possibly be a mile or more, probably not less than a ten-thousandth of the whole distance; and the *direction* of the line is uncertain in about the same degree. Of course, on those portions of either continent which have been directly connected with each other by geodetic triangulations, no corresponding uncertainty obtains; and as time goes on, and these surveys are extended, the form and dimensions of each continuous land-surface will become more and more perfectly determined. But at present we have no satisfactory means of obtaining the desired accuracy in the relative position of places separated by oceans, so that they cannot be connected by chains of triangulation. Astronomical determinations of latitude and longitude do not meet the case; since, in the last analysis, they only give at any selected station the *direction of gravity* relative to the axis of the earth, and some fixed meridian plane, and do not furnish any *linear* measurement or dimension.

Of course, if the surface of the earth were an exact spheroid, and if there were no irregular attractions due to mountains and valleys and the varying density of strata, the difficulty could be easily evaded; but, as the matter stands, it looks as if nothing short of a complete geodetic triangulation of the whole earth would ever answer the purpose, — a triangulation covering Asia and Africa, as well as Europe, and brought into America by way of Siberia and Bering Strait.

It is indeed theoretically possible, and just conceivable, that the problem may some day be reversed, and that the geodesist may come to owe some of his most important data to the observers of the lunar motions. When the relative position of two or more remote observatories shall have been precisely determined by triangulation (for instance, Greenwich, Madras, and the Cape of Good Hope), and when, by improved methods and observations made at these fundamental stations, the moon's position and motion relative to them shall have been determined with an accuracy much exceeding any thing now attainable, then by similar observations, made simultaneously at any station in this hemisphere, it will be theoretically possible to determine the position of this station, and so, by way of the moon, to bridge the ocean, and ascertain how other stations are related to those which were taken as primary. I do not, of course, mean to imply, that, in the *present state* of observational astronomy, any such procedure would lead to results of much value; but, before the Asiatic

triangulation meets the American at Bering Strait, it is not unlikely that the accuracy of lunar observations will be greatly increased.

The present uncertainty as to the earth's dimensions is not, however, a sensible embarrassment to astronomers, except in dealing with the moon, especially in attempting to employ observations made at remote and ocean-separated stations for the determination of her parallax.

As to the form of the earth, it seems pretty evident that before long it will be wise to give up further attempts to determine exactly what spheroid or ellipsoid *most nearly corresponds* to the actual figure of the earth; since every new continental survey will require a modification of the elements of this spheroid in order to take account of the new data. It will be better to assume some closely approximate spheroid *as a finality*; its elements to be forever retained unchanged, while the deviations of the actual surface from this ideal standard will be the subject of continued investigation and measurement.

A more important and anxious question of the modern astronomer is, Is the earth's rotation uniform, and, if not, in what way and to what extent does it vary? The importance, of course, lies in the fact that this rotation furnishes our fundamental measure and unit of time.

Up to a comparatively recent date, there has not been reason to suspect this unit of any variation sufficient to be detected by human observation. It has long been perceived, of course, that any changes in the earth's form or dimensions must alter the length of the day. The displacement of the surface or strata by earthquakes or by more gradual elevation and subsidence, the transportation of matter towards or from the equator by rivers or ocean currents, the accumulation or removal of ice in the polar regions or on mountain-tops, — any such causes must necessarily produce a real effect. So, also, must the friction of tides and trade-winds. But it has been supposed that these effects were so minute, and to such an extent mutually compensatory, as to be quite beyond the reach of observation; nor is it yet certain that they are not. All that can be said is, that it is now beginning to be *questionable* whether they are, or are not.

The reason for suspecting perceptible variation in the earth's revolution, lies mainly in certain unexplained irregularities in the apparent motions of the moon. She alone, of all the heavenly bodies, changes her place in the sky so rapidly, that minute inaccuracies of a second or two in the time of observation would lead to sensible discrepancies in the observed position; an error of one second, in the time, corresponding to about half a second in her place, — a quantity minute, certainly, but perfectly observable. No other heavenly body has an apparent movement anywhere nearly as rapid, excepting only the inner satellite of Mars; and this body is so minute that its accurate observation is impracticable, except with the largest telescopes, and at the times when Mars is unusually near the earth.

Now, of late, the motions of the moon have been

very carefully investigated, both theoretically and observationally; and, in spite of every thing, there remain discrepancies which defy explanation. We are compelled to admit one of three things, — either the lunar theory is in some degree mathematically incomplete, and fails to represent accurately the gravitational action of the earth and sun, and other known heavenly bodies, upon her movements; or some unknown force other than the gravitational attractions of these bodies is operating in the case; or else, finally, the earth's rotational motion is more or less irregular, and so affects the time-reckoning, and confounds prediction.

If the last is really the case, it is in some sense a most discouraging fact, necessarily putting a limit to the accuracy of all prediction, unless some other unchanging and convenient measure of time shall be found to replace the 'day' and 'second.'

The question at once presents itself, How can the constancy of the day be tested? The lunar motions furnish grounds of suspicion, but nothing more; since it is at least as likely that the mathematical theory is minutely incorrect or incomplete as that the day is sensibly variable.

Up to the present time, the most effective tests suggested are from the transits of Mercury and from the eclipses of Jupiter's satellites. On the whole, the result of Professor Newcomb's elaborate and exhaustive investigation of all the observed transits, together with all the available eclipses and occultations of stars, tends rather to establish the sensible constancy of the day, and to make it pretty certain (to use his own language) that "inequalities in the lunar motions, not accounted for by the theory of gravitation, really exist, and in such a way that the mean motion of the moon between 1800 and 1875 was really less (i.e., slower) than between 1720 and 1800." Until lately, the observations of Jupiter's satellites have not been made with sufficient accuracy to be of any use in settling so delicate a question; but at present the observation of their eclipses is being carried on at Cambridge, Mass., and elsewhere, by methods that promise a great increase of accuracy over any thing preceding. Of course, no *speedy* solution of the problem is possible through such observations, and their result will not be so free from mathematical complications as desirable, — complications arising from the mutual action of the satellites, and the ellipsoidal form of the planet. On account of its freedom from all sensible disturbances, the remote and lonely satellite of Neptune may possibly some time contribute useful data to the problem.

We have not time, and it lies outside my present scope, to discuss whether, and, if so, how, it may be possible to find a unit of time (and length) which shall be independent of the earth's conditions and dimensions (free from all *local considerations*), cosmical, and as applicable in the planetary system of the remotest star as in our own. At present we can postpone its consideration; but the time must unquestionably come, when the accuracy of scientific observation will be so far increased, that the irregularities of the earth's rotation, produced by the causes

alluded to a few minutes ago, will protrude, and become intolerable. Then a new unit of time will have to be found for scientific purposes, founded, perhaps, as has been already suggested by many physicists, upon the vibrations or motion of light, or upon some other physical action which pervades the universe.

Another problem of terrestrial astronomy relates to the constancy of the position of the earth's axis in the globe. Just as displacements of matter upon the surface or in the interior of the earth would produce changes in the time of rotation, so also would they cause corresponding alterations in the position of the axis and in the places of the poles, — changes certainly very minute. The only question is, whether they are so minute as to defy detection. It is easy to see that any such displacements of the earth's axis will be indicated by changes in the *latitudes* of our observatories. If, for instance, the pole were moved a hundred feet from its present position, towards the continent of Europe, the latitudes of European observatories would be increased about one second, while in Asia and America the effects would be trifling.

The only observational evidence of such movements of the pole, which thus far amounts to any thing, is found in the results obtained by Nyren in reducing the determinations of the latitude of Pulkowa, made with the great vertical circle, during the last twenty-five years. They seem to show a slow, steady diminution of the latitude of this observatory, amounting to about a second in a century; as if the north pole were drifting away, and increasing its distance from Pulkowa at the rate of about one foot a year.

The Greenwich and Paris observations do not show any such result; but they are not conclusive, on account of the difference of longitude, to say nothing of their inferior precision. The question is certainly a doubtful one; but it is considered of so much importance, that, at the meeting of the International geodetic association in Rome last year, a resolution was adopted recommending observations specially designed to settle it. The plan of Sig. Fergola, who introduced the resolution, is to select pairs of stations, having nearly the same latitude, but differing widely in longitude, and to determine the *difference* of their latitudes by observations of the same set of stars, observed with similar instruments, in the same manner, and reduced by the same methods and formulæ. So far as possible, the same observers are to be retained through a series of years, and are frequently to exchange stations when practicable, so as to eliminate personal equations. The main difficulty of the problem lies, of course, in the minuteness of the effect to be detected; and the only hope of success lies in the most scrupulous care and precision in all the operations involved.

Other problems, relating to the rigidity of the earth and its internal constitution and temperature, have, indeed, astronomical bearings, and may be reached to some extent by astronomical methods and considerations; but they lie on the border of our science, and

time forbids any thing more than their mere mention here.

If we consider, next, the problems set us by the moon, we find them numerous, important, and difficult. A portion of them are purely mathematical, relating to her orbital motion; while others are physical, and have to do with her surface, atmosphere, heat, etc.

As has been already intimated, the lunar theory is not in a satisfactory state. I do not mean, of course, that the moon's deviations from the predicted path are gross and palpable, — such, for instance, as could be perceived by the unaided eye (this I say for the benefit of those who otherwise might not understand how small a matter sets astronomers to grumbling); but they are large enough to be easily observable, and even obtrusive, amounting to several seconds of arc, or miles of space. As we have seen, the attempt to account for them by the irregularity of the earth's rotation has apparently failed; and we are driven to the conclusion, either that other forces than gravitation are operative upon the lunar motions, or else (what is far more probable, considering the past history of theoretical astronomy) that the mathematical theory is somewhere at fault.

To one looking at the matter a little from the outside, it seems as if that which is most needed just now, in order to secure the advance of science in many directions, is a new, more comprehensive, and more manageable solution of the fundamental equations of motion under attraction. Far be it from me to cry out against those mathematicians who delight themselves in transcendental and *n*-dimensional space, and revel in the theory of numbers, — we all know how unexpectedly discoveries and new ideas belonging to one field of science find use and application in widely different regions, — but I own, I feel much more interest in the study of the theory of functions and differential equations, and expect more aid for astronomy from it.

The problem of any number of bodies, moving under their mutual attraction, according to the Newtonian laws, stands, from a physical point of view, on precisely the same footing as that of *two* bodies. Given the masses, and the positions and velocities corresponding to any moment of time, then the whole configuration of the system for all time, past and future (abstracting outside forces, of course), is absolutely determinate, and amenable to calculation. But while, in the case of *two* bodies, the calculation is easy and feasible, by methods known for two hundred years, our analysis has not yet mastered the general problem for more than two. In special instances, by computations, tedious, indirect, and approximate, we can, indeed, carry our predictions forward over long periods, or indicate past conditions with any required degree of accuracy; but a general and universally practicable solution is yet wanting. The difficulties in the way are purely mathematical: a step needs to be taken, corresponding in importance to the introduction of the circular functions, into trigonometry, the invention of logarithms, or the discovery of the calculus. The problem confronts the

astronomer on a hundred different roads; and, until it is overcome, progress in these directions must be slow and painful. One could not truly say, perhaps, that the lunar theory must, in the mean while, remain quite at a standstill: labor expended in the old ways, upon the extension and development of existing methods, may not be fruitless, and may, perhaps, after a while, effect the reconciliation of prediction and observation far beyond the present limits of accuracy. But if we only had the mathematical powers we long for, then progress would be as by wings: we should fly, where now we crawl.

As to the physical problems presented by the moon, the questions relating to the light and heat—the radiant energy—it sends us, and to its temperature, seem to be the most attractive at present, especially for the reason that the results of the most recent investigators seem partially to contradict those obtained by their predecessors some years ago. It now looks as if we should have to admit that nearly all we receive from the moon is simply *reflected* sun-light and sun-heat, and that the temperature of the lunar surface nowhere rises as high as the freezing-point of water, or even of mercury. At the same time, some astronomers of reputation are not disposed to admit such an upsetting of long-received ideas; and it is quite certain, that, in the course of the next few years, the subject will be carefully and variously investigated.

Closely connected with this is the problem of a lunar atmosphere—if, indeed, she has any.

Then there is the very interesting discussion concerning changes upon the moon's surface. Considering the difference between our modern telescopes and those employed fifty or a hundred years ago, I think it still far from certain that the differences between the representations of earlier and later observers necessarily imply any real alterations. But they, no doubt, render it considerably *probable* that such alterations have occurred, and are still in progress; and they justify a persistent, careful, minute, and thorough study of the details of the lunar surface with powerful instruments: especially do they inculcate the value of large-scale photographs, which can be preserved for future comparison as unimpeachable witnesses.

I will not leave the moon without a word in respect to the remarkable speculations of Professor George Darwin concerning the tidal evolution of our satellite. Without necessarily admitting all the numerical results as to her age and her past and future history, one may certainly say that he has given a most plausible and satisfactory explanation of the manner in which the present state of things might have come about through the operation of causes known and recognized, has opened a new field of research, and shown the way to new dominions. The introduction of the doctrine of the conservation of energy, as a means of establishing the conditions of motion and configuration in an astronomical system, is a very important step.

In the planetary system we meet, in the main, the same problems as those that relate to the moon, with a few cases of special interest.

For the most part, the accordance between theory and observation in the motions of the larger planets is as close as could be expected. The labors of Leverrier, Hill, Newcomb, and others, have so nearly cleared the field, that it seems likely that several decades will be needed to develop discrepancies sufficient to furnish any important corrections to our present tables. Leverrier himself, however, indicated one striking and significant exception to the general tractableness of the planets. Mercury, the nearest to the sun, and the one, therefore, which ought to be the best behaved of all, is rebellious to a certain extent: the perihelion of its orbit moves around the sun more rapidly than can be explained by the action of the other known planets. The evidence to this effect has been continually accumulating ever since Leverrier first announced the fact, some thirty years ago; and the recent investigation by Professor Newcomb, of the whole series of observed transits, puts the thing beyond question. Leverrier's own belief (in which he died) was, that the effect is due to an unknown planet or planets between Mercury and the sun; but, as things now stand, we think that any candid investigator must admit that the probability of the existence of any such body or bodies of considerable dimensions is vanishingly small. We do not forget the numerous instances of round spots seen on the solar disk, nor the eclipse-stars of Watson, Swift, Trouvelot, and others; but the demonstrated possibility of error or mistake in all these cases, and the tremendous array of negative evidence from the most trustworthy observers, with the best equipment and opportunity, makes it little short of certain that there is no Vulcan in the planetary system.

A ring of meteoric matter between the planet and the sun might account for the motion of the perihelion; but, as Newcomb has suggested, such a ring would also disturb the *nodes* of Mercury's orbit.

It has been surmised that the cause may be something in the distribution of matter within the solar globe, or some variation in gravitation from the exact law of the inverse square, or some supplementary electric or magnetic action of the sun, or some special effect of the solar radiation, sensible on account of the planet's proximity, or something peculiar to the region in which the planet moves; but as yet no satisfactory explanation has been established.

Speaking of unknown planets, we are rather reluctantly obliged to admit that it is a part of our scientific duty as astronomers to continue to search for the remaining asteroids; at least, I suppose so, although the family has already become embarrassingly large. Still I think we are likely to learn as much about the constitution, genesis, and history of the solar system from these little flying rocks as from their larger relatives; and the theory of perturbations will be forced to rapid growth in dealing with the effects of Jupiter and Saturn upon their motions.

Nor is it unlikely that some day the searcher for these insignificant little vagabonds may be rewarded by the discovery of some great world, as yet unknown, slow moving in the outer desolation beyond the re-

mostest of the present planetary family. Some configurations in certain cometary orbits, and some almost evanescent peculiarities in Neptune's motions, have been thought to point to the existence of such a world; and there is no evidence, nor even a presumption, against it.

Mercury as yet defies all our attempts to ascertain the length of its day, and the character and condition of its surface. Apparently the instruments and methods now at command are insufficient to cope with the difficulties of the problem; and it is not easy to say how it can be successfully attacked.

With Venus, the earth's twin-sister, the state of things is a little better: we do already know, with some degree of approximation, her period of rotation; and the observations of the last few months bid fair, if followed up, to determine the position of her poles, and possibly to give us some knowledge of her mountains, continents, and seas.

It would be rash to say of Mars that we have reached the limit of possible knowledge as regards a planet's surface; but the main facts are now determined, and we have a rather surprising amount of supposed knowledge regarding his geography. By 'supposed' I mean merely to insinuate a modest doubt whether some of the map-makers have not gone into a little more elaborate detail than the circumstances warrant. At any rate, while the 'areographies' agree very well with each other in respect to the planet's more important features, they differ widely and irreconcilably in minor points.

As regards the physical features of the asteroids, we at present know practically nothing: the field is absolutely open. Whether it is worth any thing may be a question; and yet, if one *could* reach it, I am persuaded that a knowledge of the substance, form, density, rotation, temperature, and other physical characteristics, of one of these little orphans, would throw vivid light on the nature and behavior of interplanetary space, and would be of great use in establishing the physical theory of the solar system.

The planet Jupiter, lordliest of them all, still, as from the first, presents problems of the highest importance and interest. A sort of connecting-link between suns and planets, it seems as if, perhaps, we might find, in the beautiful and varied phenomena he exhibits, a kind of halfway house between familiar terrestrial facts and solar mysteries. It seems quite certain that no analogies drawn from the earth and the earth's atmosphere alone will explain the strange things seen upon his disk, some of which, especially the anomalous differences observed between the rotation periods derived from the observation of markings in different latitudes, are very similar to what we find upon the sun. 'The great red spot' which has just disappeared, after challenging for several years our best endeavors to understand and explain it, still, I think, remains as much a mystery as ever, — a mystery probably hiding within itself the master-key to the constitution of the great orb of whose inmost nature it was an outward and most characteristic expression. The same characteristics are also probably manifested in other less conspicuous but

equally curious and interesting markings on the varied and ever-changing countenance of this planet; so that, like the moon, it will well repay the most minute and assiduous study.

Its satellite system also deserves careful observation, especially in respect to the eclipses which occur; since we find in them a measure of the time required for light to cross the orbit of the earth, and so of the solar parallax, and also because, as has been already mentioned, they furnish a test of the constancy of the earth's rotation. The photometric method of observing these eclipses, first instituted by Professor Pickering at Cambridge in 1878, and since re-invented by Cornu in Paris, has already much increased the precision of the results.

With reference to the mathematical theory of the motion of these satellites, the same remarks apply as to the planetary theory. As yet nothing appears in the problem to be beyond the power and scope of existing methods, when carried out with the necessary care and prolixity; but a new and more compendious method is most desirable.

The problems of Saturn are much the same as those of Jupiter, excepting that the surface and atmospheric phenomena are less striking, and more difficult of observation. But we have, in addition, the wonderful rings, unique in the heavens, the loveliest of all telescopic objects, the type and pattern, I suppose, of world-making, in actual progress before our eyes. There seems to be continually accumulating evidence from the observations of Struve, Dawes, Henry, and others, that these whirling clouds are changing in their dimensions and in the density of their different parts; and it is certainly the duty of every one who has a good telescope, a sharp eye, and a chastened imagination, to watch them carefully, and set down exactly what he sees. It may well be that even a few decades will develop most important and instructive phenomena in this gauzy girdle of old Chronos. Great care, however, is needed in order not to mistake fancies and illusions for solid facts. Not a few anomalous appearances have been described and commented on, which failed to be recognized by more cautious observers with less vivid imaginations, more trustworthy eyes, and better telescopes.

The outer planets, Uranus and Neptune, until recently, have defied all attempts to study their surface and physical characteristics. Their own motions and those of their satellites, have been well worked out; but it remains to discuss their rotation, topography, and atmospheric peculiarities. So remote are they, and so faintly illuminated, that the task seems almost hopeless; and yet, within the last year or two, some of our great telescopes have revealed faint and evanescent markings upon Uranus, which may in time lead to some further knowledge of that far-off relative. It may, perhaps, be that some great telescope of the future will give us some such views of Neptune as we now get of Jupiter.

There is a special reason for attempts to determine the rotation periods of the planets, in the fact that there is very possibly some connection between these

periods, on the one hand, and, on the other, the planets' distances from the sun, their diameters and masses. More than thirty years ago, Professor Kirkwood supposed that he had discovered the relation in the analogy which bears his name. The materials for testing and establishing it were then, however, insufficient, and still remain so, leaving far too many of the data uncertain and arbitrary. Could such a relation be discovered, it could hardly fail to have a most important significance with respect to theories of the origin and development of the planetary system.

The great problem of the absolute dimensions of our system is, of course, commanded by the special problem of the solar parallax; and this remains a problem still. Constant errors of one kind or another, the origin of which is still obscure, seem to affect the different methods of solution. Thus, while experiments upon the velocity of light and heliometric measurements of the displacements of Mars among the stars agree remarkably in assigning a smaller parallax (and greater distance of the sun) than seems to be indicated by the observations of the late transits of Venus, and by methods founded on the lunar motions, on the other hand, the meridian observations of Mars all point to a larger parallax and smaller distance. While still disposed to put more confidence in the methods first named, I, for one, must admit that the margin of probable error seems to me to have been rather increased than diminished by the latest published results deduced from the transits. I do not feel so confident of the correctness of the value $8''.80$ for the solar parallax as I did three years ago. In its very nature, this problem is one, however, that astronomers can never have done with. So fundamental is it, that the time will never come when they can properly give up the attempt to increase the precision of their determination, and to test the received value by every new method that may be found.

The problems presented by the sun alone might themselves well occupy more than the time at our disposal this evening. Its mass, dimensions, and motions, as a whole, are, indeed, pretty well determined and understood; but when we come to questions relating to its constitution, the cause and nature of the appearances presented upon its surface, the periodicity of its spots, its temperature, and the maintenance of its heat, the extent of its atmosphere, and the nature of the corona, we find the most radical differences of opinion.

The difficulties of all solar problems are, of course, greatly enhanced by the enormous difference between solar conditions and the conditions attainable in our laboratories. We often reach, indeed, similarity sufficient to establish a bond of connection, and to afford a basis for speculation; but the dissimilarity remains so great as to render quantitative calculations unsafe, and make positive conclusions more or less insecure. We can pretty confidently infer the presence of iron and hydrogen and other elements in the sun by appearances which we can reproduce upon the earth; but we cannot safely apply empirical formulae (like

that of Dulong and Petit, for instance), deduced from terrestrial experiments, to determine solar temperatures: such a proceeding is an unsound and unwarrantable extrapolation, likely to lead to widely erroneous conclusions.

For my own part, I feel satisfied as to the substantial correctness of the generally received theory of the sun's constitution, which regards this body as a great ball of intensely heated vapors and gases, clothed outwardly with a coat of dazzling clouds formed by the condensation of the less volatile substances into drops and crystals like rain and snow. Yet it must be acknowledged that this hypothesis is called in question by high authorities, who maintain, with Kirchhoff and Zöllner, that the visible photosphere is no mere layer of clouds, but either a solid crust, or a liquid ocean of molten metals; and there may be some who continue to hold the view of the elder Herschel (still quoted as authoritative in numerous school-books), that the central core of the sun is a solid and even habitable globe, having the outer surface of its atmosphere covered with a sheet of flame maintained by some action of the matter diffused in the space through which the system is rushing. We must admit that the question of the sun's constitution is not yet beyond debate.

And not only the constitution of the sun itself, but the nature and condition of the matter composing it, is open to question. Have we to do with iron and sodium and hydrogen as we know them on the earth, or are the solar substances in some different and more elemental state?

However confident many of us may be as to the general theory of the constitution of the sun, very few, I imagine, would maintain that the full explanation of sun-spots and their behavior has yet been reached. We meet continually with phenomena, which, if not really contradictory to prevalent ideas, at least do not find in them an easy explanation.

So far as mere visual appearances are concerned, I think it must be conceded, that the most natural conception is that of a dark chip or scale thrown up from beneath, like scum in a caldron, and floating, partly submerged, in the blazing flames of the photosphere which overhang its edges, and bridge across it, and cover it with filmy veils, until at last it settles down again and disappears. It hardly *looks* like a mere hollow filled with cooler vapor, nor is its appearance that of a cyclone seen from above. But then, on the other hand, its spectrum under high dispersion is very peculiar; not at all that of a solid, heated slag, but it is made up of countless fine dark lines, packed almost in contact, showing, however, here and there, a bright line, or at least an interspace where the rank is broken by an interval wider than that which elsewhere separates the elementary lines,—a spectrum, which, so far as I know, has not yet found an analogue in any laboratory experiment. It seems, however, to belong to the type of absorption spectra, and to indicate, as the accepted theory requires, that the spot is dark in consequence of *loss* of light, and not from any original defect of luminosity. Here, certainly, are problems that require solution.

The problem of the sun's peculiar rotation and

equatorial acceleration is a most important one and still unsolved. Probably its solution depends in some way upon a correct understanding of the exchanges of matter going on between the interior and the surface of the fluid, cooling globe. It is a significant fact (already alluded to), that a similar relation appears to hold upon the disk of Jupiter; the bright spots near the equator of the planet completing their rotation about five minutes more quickly than the great red spot which was forty degrees from the equator. It is hardly necessary to say that an astronomer, watching our terrestrial clouds from some external station (on the moon, for instance), would observe just the reverse. Equatorial clouds would complete their revolution more slowly than those in our own latitude. Our storms travel toward the east, while the volcanic dust from Krakatoa moved swiftly west. We may at least conjecture that the difference between different planets somehow turns upon the question, whether the body whose atmospheric currents we observe is receiving more heat from without than it is throwing off itself. Whatever may be the true explanation of this peculiarity in the motion of sun-spots, it will, when reached, probably carry with it the solution of many other mysteries, and will arbitrate conclusively between rival hypotheses.

The periodicity of the sun-spots suggests a number of important and interesting problems; relating, on the one hand, to its mysterious cause, and, on the other, to the possible effects of this periodicity upon the earth and its inhabitants. I am no 'sun-spottist' myself, and am more than sceptical whether the terrestrial influence of sun-spots amounts to any thing worth speaking of, except in the direction of magnetism. But all must concede that this is by no means yet demonstrated (it is not easy to prove a negative); and there certainly are facts and presumptions enough tending the other way to warrant more extended investigation of the subject. The investigation is embarrassed by the circumstance, pointed out by Dr. Gould, that the effects of sun-spot periodicity, if they exist at all (as he maintains they do), are likely to be quite different in different portions of the earth. The influence of changes in the amount of the solar radiation will, he says, be first and chiefly felt in alterations and deflections of the prevailing winds, thus varying the *distribution* of heat and rain upon the surface of the earth without necessarily much changing its absolute amount. In some regions it may, therefore, be warmer and dryer during a sun-spot maximum, while in adjoining countries it is the reverse.

There can be no question, that it is now one of the most important and pressing problems of observational astronomy to devise apparatus and methods delicate enough to enable the student to follow promptly and accurately the presumable changes in the daily, even the hourly, amounts of the solar radiation. It might, perhaps, be possible with existing instruments to obtain results of extreme value from observations kept up with persistence and scrupulous care for several years at the top of some rainless

mountain, if such can be found; but the undertaking would be a difficult and serious affair, quite beyond any private means.

Related to this subject is the problem of the connection between the activity of the solar surface and magnetic disturbances on the earth, — a connection unquestionable as matter of fact, but at present unexplained as matter of theory. It may have something to do with the remarkable prominence of iron in the list of solar materials; or the explanation may, perhaps, be found in the mechanism by means of which the radiations of light and heat traverse interplanetary space, presenting itself ultimately as a corollary of the perfected electro-magnetic theory of light.

The chromosphere and prominences present several problems of interest. One of the most fruitful of them relates to the spectroscopic phenomena at the base of the chromosphere, and especially to the strange differences in the behavior of different spectrum-lines, which, according to terrestrial observations, are due to the same material. Of two lines (of iron, for instance) side by side in the spectrum, one will glow and blaze, while the other will sulk in imperturbable darkness; one will be distorted and shattered, presumably by the swift motion of the iron vapor to which it is due, while the other stands stiff and straight.

Evidently there is some deep-lying cause for such differences; and as yet no satisfactory explanation appears to me to have been reached, though much ingenious speculation has been expended upon it. Mr. Lockyer's bold and fertile hypothesis, already alluded to, that at solar and stellar temperatures our elements are decomposed into others more elemental yet, seems to have failed of demonstration thus far, and rather to have lost ground of late; and yet one is almost tempted to say, 'It *ought* to be true,' and to add that there is more than a possibility that its essential truth will be established some time in the future.

Probably all that can be safely said at present is, that the spectrum of a metallic vapor (iron, for instance, as before) depends not only upon the chemical element concerned, but also upon its physical conditions; so that, at different levels in the solar atmosphere, the spectrum of the iron will differ greatly as regards the relative conspicuousness of different lines; and so it will happen, that, whenever any mass of iron vapor is suffering disturbance, those lines only which particularly characterize the spectrum of iron in that special state will be distorted or reversed, while all their sisters will remain serene.

The problem of the solar corona is at present receiving much attention. The most recent investigations respecting it — those of Dr. Huggins and Professor Hastings — tend in directions which appear to be diametrically opposite. Dr. Huggins considers that he has succeeded in photographing the corona in full sunshine, and so in establishing its objective reality as an immense solar appendage, sub-permanent in form, and rotating with the globe to which it is attached. One may call it 'an atmosphere,' if the

word is not to be too rigidly interpreted. I am bound to say that plates which he has obtained do really show just such appearances as would be produced by such a solar appendage, though they are very faint and ghost-like. I may add further, that, from a letter from Dr. Huggins, recently received, I learn that he has been prevented from obtaining any similar plates in England this summer by the atmospheric haze, but that Dr. Woods, who has been provided with a similar apparatus, and sent to the Riffelberg in Switzerland, writes that he has 'an assured success.'

Our American astronomer, on the other hand, at the last eclipse (in the Pacific Ocean), observed certain phenomena which seem to confirm a theory he had formulated some time ago, and to indicate that the lovely apparition is an apparition only, a purely optical effect due to the *diffraction* (not *refraction*, nor reflection either) of light at the edge of the moon—no more a solar appendage than a rainbow or a mock sun. There are mathematical considerations connected with the theory which may prove decisive when the paper of its ingenious and able proposer comes to be published in full. In the mean time it must be frankly conceded that the observations made by him are very awkward to explain on any other hypothesis.

Whatever may be the result, the investigation of the status and possible extent of a nebulous envelope around a sun or a star is unquestionably a problem of very great interest and importance. We shall be compelled, I believe, as in the case of comets, to recognize other forces than gravity, heat, and ordinary gaseous elasticity, as concerned in the phenomena. As regards the actual existence of an extensive gaseous envelope around the sun, I may add that other appearances than those seen at an eclipse seem to demonstrate it beyond question,—phenomena such as the original formation of clouds of incandescent hydrogen at high elevations, and the forms and motions of the loftiest prominences.

But of all solar problems, the one which excites the deepest and most general interest is that relating to the solar heat, its maintenance and its duration. For my own part, I find no fault with the solution proposed by Helmholtz, who accounts for it mainly by the slow contraction of the solar sphere. The only objection of much force is, that it apparently limits the past duration of the solar system to a period not much exceeding some twenty millions of years; and many of our geological friends protest against so scanty an allowance. The same theory would give us, perhaps, half as much time for our remaining lifetime; but this is no objection, since I perceive no reason to doubt the final cessation of the sun's activity, and the consequent death of the system. But while this hypothesis seems fairly to meet the requirements of the case, and to be a necessary consequence of the best knowledge we can obtain as to the genesis of our system and the constitution of the sun itself, it must, of course, be conceded that it does not yet admit of any observational verification. No measurements within our power can test it, so far as we can see at present.

It may be admitted, too, that much can be said in favor of other theories; such as the one which attributes the solar heat to the impact of meteoric matter, and that other most interesting and ingenious theory of the late Sir William Siemens.

As regards the former, however, I see no escape from the conclusion, that, if it were exclusively true, the earth ought to be receiving, as was pointed out by the late Professor Peirce, as much heat from meteors as from the sun. This would require the fall of a quantity of meteoric matter,—more than sixty million times as much as the best estimates make our present supply, and such as could not escape the most casual observation, since it would amount to more than a hundred and fifty¹ tons a day on every square mile.

As regards the theory of Siemens, the matter has been, of late, so thoroughly discussed, that we probably need spend no time upon it here. To say nothing as to the difficulties connected with the establishment of such a far-reaching vortex as it demands, nor of the fact that the temperature of the sun's surface appears to be above that of the dissociation point of carbon compounds, and hence above the highest heat of their combustion, it seems certainly demonstrated, that matter of the necessary density could not exist in interplanetary space without seriously affecting the planetary motions by its gravitating action as well as by its direct resistance; nor could the stellar radiations reach us, as they do, through a medium capable of taking up and utilizing the rays of the sun in the way this theory supposes.

And yet I imagine that there is a very general sympathy with the feeling that led to the proposal of the theory,—an uncomfortable dissatisfaction with received theories, because they admit that the greater part of the sun's radiant energy is, speaking from a scientific point of view, simply wasted. Nothing like a millionth part of the sky, as seen from the sun, is occupied, so far as we can make out, by objects upon which its rays can fall: the rest is vacancy. If the sun sends out rays in all directions alike, not one of them in a million finds a target, or accomplishes any useful work, unless there is in space some medium to utilize the rays, or unknown worlds of which we have no cognizance beyond the stars.

Now, for my own part, I am very little troubled by accusations of wastefulness against nature, or by demands for theories which will show what the human mind can recognize as 'use' for all energy expended. Where I can perceive such use, I recognize it with reverence and gratitude, I hope; but the

¹ In an article on astronomical collisions, published in the *North-American review* about a year ago, I wrongly stated the amount at fifty tons. There was some fatality connected with my calculations for that article. I gave the amount of heat due to the five hundred tons of meteoric matter which is supposed to fall daily on the earth with an average velocity of fifteen miles per second as fifty-three calories annually per square metre,—a quantity two thousand times too great. Probably the error would have been noticed if even the number given had not been so small, compared with the solar heat, as fully to justify my argument, which is only strengthened by the correction. I owe the correction to Professor LeConte of California, who called my attention to the errors.

failure to recognize it in other cases creates in my mind no presumption against the wisdom of nature, or against the correctness of an hypothesis otherwise satisfactory. It merely suggests human limitations and ignorance. How can one without sight understand what a telescope is good for?

At the same time, perhaps we assume with a little too much confidence, that, in free space, radiation does take place equally in all directions. Of course, if the received views as to the nature and conduct of the hypothetical 'ether' are correct, there is no possibility of questioning the assumption; but, as Sir John Herschel and others have pointed out, the properties which must be ascribed to this 'ether,' to fit it for its various functions, are so surprising and almost inconceivable, that one may be pardoned for some reserve in accepting it as a finality. At any rate, as a fact, the question is continually started (the idea has been brought out repeatedly, in some cases by men of real and recognized scientific and philosophic attainment), whether the constitution of things may not be such that radiation and transfer of energy can take place only between ponderable masses; and that, too, without the expenditure of energy upon the transmitting-agent (if such exist) along the line of transmission, even *in transitu*. If this were the case, then, the sun would send out its energy only to planets and meteors and sister-stars, wasting none in empty space; and so its loss of heat would be enormously diminished, and the time-scale of the life of the planetary system would be correspondingly extended. So far as I know, no one has ever yet been able to indicate any kind of medium or mechanism by which vibrations, such as we know to constitute the radiant energy of light and heat, can be transmitted at all from sun to planet under such restrictions. Still one ought not to be too positive in assertions as to the real condition and occupancy of so-called vacant space. The 'ether' is a good working hypothesis, but hardly more as yet.

I need not add, that a most interesting and as yet inaccessible problem, connected with the preceding, is that of the mechanism of gravitation, and, indeed, of all forces that seem to act at a distance: as for that matter, in the last analysis, *all* forces do. If there really be an 'ether,' then it would seem that somehow all attractions and repulsions of ponderable matter must be due to its action. Challis's investigations and conclusions as to the effect of hydrodynamic actions in such a medium do not seem to have commanded general acceptance; and the field still lies open for one who will show how gravitation and other forces can be correlated with each other through the ether.

Meteors and the comets, seeming to belong neither to the solar system nor to the stellar universe, present a crowd of problems as difficult as they are interesting. Much has undoubtedly been gained during the last few decades, but in some respects that which has been learned has only deepened the mystery.

The problem of the origin of comets has been supposed to be solved to a certain extent by the researches of Schiaparelli, Heis, Professor Newton,

and others, who consider them to be strangers coming in from outer space, sometimes 'captured' by planets, and forced into elliptic orbits, so as to become periodic in their motion. Certainly this theory has strong supports and great authority, and probably it meets the conditions better than any other yet proposed. But the objections are really great, if not insuperable, — the fact that we have so few, if any, comets moving in hyperbolic orbits, as comets *met* by the sun would be expected to move; that there seems to be so little relation between the direction of the major axes of cometary orbits, and the direction of the solar motion in space; and especially the fact, pointed out and insisted upon by Mr. Proctor in a recent article, that the alteration of a comet's natural parabolic orbit to the observed elliptic one, by planetary action, implies a reduction of the comet's velocity greater than can be reasonably explained. If, for instance, Brorsen's comet (which has a mean distance from the sun a little more than three times that of the earth) was really once a parabolic comet, and was diverted into its present path by the attraction of Jupiter, as generally admitted, it must have had its velocity reduced from about eleven miles a second to five. Now, it is very difficult, if not out of the question, to imagine any possible configuration of the two bodies and their orbits which could result in so great a change. While I am by no means prepared to indorse as conclusive all the reasoning in the article referred to, and should be very far from ready to accept the author's alternative theory (that the periodic comets have been ejected from the planets, and so are not their captives, but their children), I still feel that the difficulty urged against the received theory is very real, and not to be evaded, though it may possibly be overcome by future research.

Still more problematical is the constitution of these strange objects of such enormous volume and inconceivable tenuity, self-luminous and transparent, yet reflecting light, the seat of forces and phenomena unparalleled in all our other experience. Hardly a topic relating to their appearance and behavior can be named which does not contain an unsolved problem. The varying intensity, polarization, and spectroscopic character of their light; the configurations of the nucleus and its surrounding nebulousity; and especially the phenomena of jets, envelopes, and tail, — all demand careful observation and thorough discussion.

I think it may be regarded as certain, that the explanation of these phenomena when finally reached, if that time ever comes, will carry with it, and be based upon, an enormous increase in our knowledge as to the condition, contents, and temperature of interplanetary space, and the behavior of matter when reduced to lowest terms of density and temperature.

Time forbids any adequate discussion of the numerous problems of stellar astronomy. Our work, in its very nature incessant and interminable, consists, of course, in the continual observation and cataloguing of the places of the stars, with ever-increasing precision. These star-places form the scaffold and framework of all other astronomical

investigations involving the motions of the heavenly bodies: they are the reference-points and benchmarks of the universe. Ultimately, too, the comparison of catalogues of different dates will reveal the paths and motions of all the members of the starry host, and bring out the great orbit of the sun and his attendant planets.

Meanwhile, micrometric observations are in order, upon the individual stars in different clusters, to ascertain the motions which occur in such a case; and the mathematician is called upon again to solve the problem of such movement.

Now, too, since the recent work of Gill and Elkin in South Africa, and of Struve, Hall, and others, elsewhere, upon stellar parallax, new hopes arise that we may soon come to some wider knowledge of the subject; that, instead of a dozen or so parallaxes of doubtful precision, we may get a hundred or more relating to stars of widely different brightness and motion, and so be enabled to reach some trustworthy generalizations as to the constitution and dimensions of the stellar universe, and the actual rates of stellar and solar motion in space.

Most interesting, also, are the studies now so vigorously prosecuted by Professor Pickering in this country, and many others elsewhere, upon the brightness of the stars, and the continual variations in this brightness. Since 1875, stellar photometry has become almost a new science.

Then, there are more than a myriad of double and multiple stars to watch, and their orbits to be determined; and the nebulae claim keen attention, since some of them appear to be changing in form and brightness, and are likely to reveal to us some wonderful secrets in the embryology of worlds.

Each star also presents a subject for spectroscopic study; for although, for the most part, the stars may be grouped into a very few classes from the spectroscopic point of view, yet, in detail, the spectra of objects belonging to the same group differ considerably and significantly, almost as much as human faces do.

For such investigations, new instruments are needed, of unexampled powers and accuracy, some for angular measurement, some for mere power of seeing. Photography comes continually more and more to the front; and the idea sometimes suggests itself, that by and by the human eye will hardly be trusted any longer for observations of precision, but will be superseded by an honest, unprejudiced, and unimaginative plate and camera. The time is not yet, however, most certainly. Indeed, it can never come at all, as relates to certain observations; since the human eye and mind together integrate, so to speak, the impressions of many separate and selected moments into one general view, while the camera can only give a brutal copy of an unselected state of things, with all its atmospheric and other imperfections.

New methods are also needed, I think (they are unquestionably possible), for freeing time-observations from the errors of personal equation; and increased precision is demanded, and is being progressively attained, in the prevention, or elimination, of instrumental errors, due to differences of temperature, to

mechanical strains, and to inaccuracies of construction. Astronomers are now coming to the investigation of quantities so minute, that they would be completely masked by errors of observation that formerly were usual and tolerable. The science has reached a stage, where, as was indicated at the beginning of this address, it has to confront and deal with the possible unsteadiness of the earth's rotation, and the instability of its axis. The astronomer has now to reverse the old maxim of the courts: for him, and most emphatically at present, *de minimis curat lex*. Residuals and minute discrepancies are the seeds of future knowledge, and the very foundations of new laws.

And now, in closing this hurried and inadequate, but I fear rather tedious, review of the chief problems that are at present occupying the astronomer, what answer can we give to him who insists, *Cui bono?* and requires a reason for the enthusiasm that makes the votaries of our science so ardent and tireless in its pursuit? Evidently very few of the questions which have been presented have much to do directly with the material welfare of the human race. It may possibly turn out, perhaps, that the investigation of the solar radiation, and the behavior of sun-spots, may lead to some better understanding of terrestrial meteorology, and so aid agricultural operations and navigation. I do not say it will be so, — in fact, I hardly expect it, — but I am not sure it will not. Possibly, too, some few other astronomical investigations may facilitate the determination of latitudes and longitudes, and so help exploration and commerce; but, with a few exceptions, it must be admitted that modern astronomical investigations have not the slightest immediate commercial value.

Now, I am not one of those who despise a scientific truth or principle because it admits of an available application to the affairs of what is called 'practical life,' and so is worth something to the community in dollars and cents: its commercial value is — just what it is — to be accepted gratefully.

Indirectly, however, almost all scientific truth has real commercial value, because 'knowledge is power,' and because (I quote it not irreverently) 'the truth shall make you free,' — any truth, and to some extent; that is to say, the intelligent and intellectually cultivated will generally obtain a more comfortable livelihood, and do it more easily, than the stupid and the ignorant. Intelligence and brains are most powerful allies of strength and hands in the struggle for existence; and so, on purely economical grounds, all kinds of science are worthy of cultivation.

But I should be ashamed to rest on this lower ground: the highest value of scientific truth is not economic, but different and more noble; and, to a certain and great degree, its truest worth is more as an object of pursuit than of possession. The 'practical life' — the eating and the drinking, the clothing and the sheltering — comes *first*, of course, and is the necessary foundation of any thing higher; but it is not the whole or the best or the most of life. Apart from all spiritual and religious considerations, which lie one side of our relations in this association, there

can be no need, before this audience, to plead the higher rank of the intellectual, aesthetic, and moral life above the material, or to argue that the pabulum of the mind is worth as much as food for the body. Now, it is unquestionable, that, in the investigation and discovery of the secrets and mysteries of the heavens, the human intellect finds most invigorating exercise, and most nourishing and growth-making aliment. What other scientific facts and conceptions are more effective in producing a modest, sober, truthful, and ennobling estimate of man's

just place in nature, both of his puny insignificance, regarded as a physical object, and his towering spirit, in some sense comprehending the universe itself, and so akin to the divine?

A nation oppressed by poverty, and near to starving, needs first, most certainly, the trades and occupations that will feed and clothe it. When bodily comfort has been achieved, then higher needs and wants appear; and then science, for truth's own sake, comes to be loved and honored along with poetry and art, leading men into a larger, higher, and nobler life.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

SOME DISTINCTIVE FEATURES OF THE BRITISH ASSOCIATION.

THE general plan of organization of the British and American associations for the advancement of science is the same. The English body has a 'council' corresponding closely to our standing committee, and a 'general committee' corresponding to our body of fellows. There are, however, many points of difference which it is well worth while to study with a view of seeing what suggestions of value we may derive for our own guidance. The first of these is, that the British association has long since given up the practice of meeting and transacting business as an organized body. The general meetings are held only to hear such papers as the president's annual address, and not to transact business of any kind. The transaction of all business by the several committees saves much time which the American association spends in the work of organizing the meeting, and electing new members; and we may expect, that, as our numbers increase, this work will become so cumbrous that we shall finally adopt some plan of putting it entirely into the hands of committees.

The organization and conduct of the scientific proceedings are so like those adopted by ourselves as to call for little remark. The division into sections is substantially the same as with us; and the main difference between the sectional programmes is that no estimated length of a paper is given by our neighbors, thus avoiding one source of deception which frequently annoys intended listeners. Perhaps it was owing to the peculiar circumstances of the meeting, that the papers and discussions were of a quality superior to what we are wont to expect in a semi-popular assemblage. Only men who had some serious object could undertake so long a journey; and

the result has been, that what such men have had to say was heard without any admixture of the crude and ignorant speculations so often interjected into the discussions, and even forming the subject of the papers. The one subject in which English thought has always been pre-eminent is the theories of physics, and the discussion on the seat of electromotive forces was all the more creditable from the barrenness of the subject. This discussion well illustrated the comparative state of science in the two countries; which may be expressed by saying, that, while a few men of the highest genius stand on the same level, foreign countries are greatly superior to us in the number of trained men, thoroughly grounded in first principles, which they are able to bring forward. The lucid statement by Professor Willard Gibbs of New Haven, of the principles involved, was the feature of the discussion; yet it would have been hard for the speaker to collect in his own country so appreciative an audience as that which greeted him from beyond the ocean.

The most valuable work of the British association has been the reports and investigations undertaken by committees of its appointment. Occasionally these reports have comprised synopses of the progress of science in special branches made by individual members, and presented to the society. The greater number have, however, been accounts of special researches, the funds for prosecuting which have been supplied by the association. A splendid example of such work, which must ever redound to the credit of the body which undertook it, is the system of electrical units now universally adopted, the basis for which was furnished by a committee of the British association. It can hardly be too much to say, that no one work of recent times has done more for the progress and diffusion of electrical

science than this. The committees are generally continued from year to year, and thus form permanent working bodies, each pursuing a definite object. Naturally, nearly all the work will be done by one or a small number of members; but the latter have the advantage and stimulus of the co-operation and advice of their fellow members, who again are in a position to provide for the continuance of the work in case the person in charge gives it up. An idea of the present importance of this feature of the organization may be gained from the fact, that there are more than forty such committees at work, reporting annually. Some examples may be cited to show the character of the work undertaken: A committee on underground temperature collects determinations of the rate of increase of temperature in mines, and other places where it is possible to determine it; another is collecting and investigating meteoric dust; another investigates the lunar disturbance of gravity; Mr. Francis Galton is at the head of a committee devising a system of statistical measurements of human beings; the economists have a committee investigating the rate of wages, and its relation to economic progress. In a word, a range of subjects from tables of binary quantics to patent legislation, and the migration of birds, are being regularly investigated. We wish there could be a body of men in this country pursuing similar objects.

The number of Americans present at the meeting was even greater than could have been expected; and the high character of the American representation is sufficiently shown by the fact that six ex-presidents of the American association were in attendance.

STEPS TOWARDS A KINETIC THEORY OF MATTER.¹

THE now well-known kinetic theory of gases is a step so important, in the way of explaining seemingly static properties of matter by motion, that it is scarcely possible to help anticipating, in idea, the arrival at a complete theory of matter, in which all its properties will be seen to be merely attributes of motion.

Rich as it is in practical results, the kinetic theory of gases, as hitherto developed, stops absolutely short at the atom or molecule, and gives not even a suggestion towards explaining the properties in virtue of which the atoms or molecules mutually influence one another.

¹ Address to the mathematical and physical section of the British association at Montreal, Aug. 28, 1884, by Professor Sir WILLIAM THOMSON, M.A., LL.D., D.C.L., F.R.S., L. & E., F.R.A.S., president of the section.

Every one who has hitherto written or done any thing very explicit in the kinetic theory of gases has taken the mutual action of molecules in collision as repulsive. May it not, after all, be attractive? Imagine a great multitude of particles enclosed by a boundary which may be pushed inwards in any part, all round, at pleasure. Now station an engineer corps of Maxwell's army of sorting demons all round the enclosure, with orders to push in the boundary diligently everywhere when none of the besieged troops are near, and to do nothing when any of them are seen approaching, and until after they have turned again inwards. The result will be, that, with exactly the same sum of kinetic and potential energies of the same enclosed multitude of particles, the throng has been caused to be denser. Now, Joule's and Thomson's old experiments on the efflux of air prove, that if the crowd be common air, or oxygen, or nitrogen, or carbonic acid, the temperature is a little higher in the denser than in the rarer condition when the energies are the same. By the hypothesis, equality of temperature between two different gases, or two portions of the same gas at different densities, means equality of kinetic energies in the same number of molecules of the two. From the observations proving the temperature to be higher, it therefore follows that the potential energy is smaller in the condensed crowd. This (always, however, under protest as to the temperature hypothesis) proves some degree of attraction among the molecules, but it does not prove ultimate attraction between two molecules in collision, or at distances much less than the average mutual distance of nearest neighbors in the multitude.

We must look distinctly on each molecule as being either a little elastic solid, or a configuration of motion in a continuous, all-pervading liquid. How we can ever permanently rest anywhere short of this last view is not evident; but it would be a very pleasant temporary resting-place on the way to it, if we could, as it were, make a mechanical model of a gas out of little pieces of round, perfectly elastic, solid matter, flying about through the space occupied by the gas, and colliding with one another, and against the sides of the containing vessel. But alas for a mechanical model consisting of the cloud of little elastic solids flying about amongst one another! Though each particle have absolutely perfect elasticity, the end must be pretty much the same as if it were but imperfectly elastic. The average effect of repeated and repeated mutual collisions must be to gradually convert all the translational energy into energy of shriller and shriller vibrations of the molecule. Even if this fatal fault in the theory did not exist, and if we could be perfectly satisfied with the kinetic theory of gases founded on the collisions of elastic solid molecules, there would still be beyond it a grander theory, which need not be considered a chimerical object of scientific ambition,—to explain the elasticity of solids.

If we could make out of matter devoid of elasticity a combined system of relatively moving parts, which, in virtue of motion, has the essential characteristics

of an elastic body, this would be at least a finger-post, pointing a way which we may hope will lead to a kinetic theory of matter. Any ideal system of material particles, acting on one another mutually through mass-less connecting springs, may be perfectly imitated in a model consisting of rigid links jointed together, and having rapidly rotating fly-wheels pivoted on some or on all of the links. The drawings (figs. 1 and 2) illustrate two such material systems. The directions of rotation of the fly-wheels in the gyrostatic

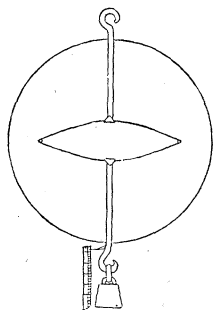


FIG. 1.

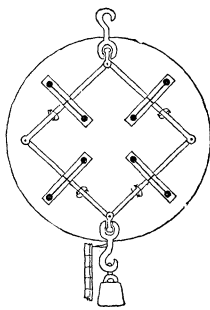


FIG. 2.

system (fig. 2) are indicated by directional ellipses, which show in perspective the direction of rotation of the fly-wheel of each gyrostat. The gyrostatic system (fig. 2) might have been constituted of two gyrostatic members, but four are shown for symmetry. The enclosing circle represents in each case, in section, an enclosing spherical shell to prevent the interior from being seen. In the inside of one there are fly-wheels; in the inside of the other, a massless spring. The projecting hooked rods seem as if they are connected by a spring in each case. If we hang any one of the systems up by the hook on one of its projecting rods, and hang a weight to the hook of the other projecting rod, the weight, when first put on, will oscillate up and down, and will go on doing so forever, if the system be absolutely unfrictional. If we check the vibration by hand, the weight will hang down at rest, the pin drawn out to a certain degree; and the distance drawn out will be simply proportional to the weight hung on, as in an ordinary spring-balance.

Here, then, out of matter possessing rigidity, but absolutely devoid of elasticity, we have made a perfect model of a spring in the form of a spring-balance. Connect millions of millions of particles by pairs of rods such as these of this spring-balance, and we have a group of particles constituting an elastic solid.

The gyrostatic model spring-balance is arranged to have zero moment of momentum as a whole, and therefore to contribute nothing to the Faraday rotation. With this arrangement, the model illustrates the luminiferous ether in a field unaffected by magnetic force. But now let there be a different rotational velocity imparted to the jointed square, round the axis of the two projecting hooked rods, such as to give a resultant moment of momentum round any given line through the centre of inertia of the system, and let pairs of the hooked rods in the model thus altered,

which is no longer a model of a mere spring-balance, be applied as connections between millions of pairs of particles, as before, with the lines of resultant moment of momentum all similarly directed: we now have a model elastic solid which will have the property that the direction of vibration in waves of rectilinear vibrations propagated through it shall turn round the line of propagation of the waves; just as Faraday's observation proves to be done by the line of vibration of light in a dense medium between the poles of a powerful magnet. The case of wave-front perpendicular to the lines of resultant moment of momentum (that is to say, the direction of propagation being parallel to these lines) corresponds, in our mechanical model, to the case of light travelling in the direction of the lines of force in a magnetic field.

But now, with the view of ultimately discarding the postulate of rigidity from all our materials, let us suppose some to be absolutely destitute of rigidity, and to possess merely inertia and incompressibility, and mutual impenetrability with reference to the still remaining rigid matter. With these postulates, we can produce a perfect model of mutual action at a distance between solid particles, fulfilling the condition, so keenly desired by Newton and Faraday, of being explained by continuous action through an intervening medium. Imagine a solid bored through with a hole, and placed in our ideal perfect liquid. For a moment let the hole be stopped by a diaphragm, and let an impulsive pressure be applied for an instant uniformly over the whole membrane, and then instantly let the membrane be dissolved into liquid. This action originates a motion of the liquid relatively to the solid, of a kind to which has been given the name of 'irrotational circulation,' which remains absolutely constant, however the solid be moved through the liquid. Thus at any time the actual motion of the liquid, at any point in the neighborhood of the solid, will be the resultant of the motion it would have in virtue of the circulation alone were the solid at rest, and the motion it would have in virtue of the motion of the solid itself had there been no circulation established through the aperture. It is interesting and important to remark, in passing, that the whole kinetic energy of the liquid is the sum of the kinetic energies which it would have in the two cases separately. Now, imagine the whole liquid to be enclosed in an infinitely large, rigid containing-vessel; and in the liquid, at an infinite distance from any part of the containing-vessel, let two perforated solids, with irrotational circulation through each, be placed at rest near one another. The resultant fluid motion due to the two circulations will give rise to fluid pressure on the two bodies, which, if unbalanced, will cause them to move.

It might be imagined that the action at a distance, thus provided for by fluid motion, could serve as a foundation for a theory of the equilibrium and the vibrations of elastic solids, and the transmission of waves like those of light through an extended quasi-elastic solid medium. But, unfortunately, the equilibrium is essentially unstable. If, however, we connect the perforated bodies, with circulation through

them in the hydrokinetic system, by jointed rigid connecting-links, we may arrange for configurations of stable equilibrium. Thus, without fly-wheels, but with fluid circulations through apertures, we may make a model spring-balance, or a model luminiferous ether, either without or with the rotational quality corresponding to that of the true luminiferous ether in the magnetic fluid; in short, do all by the perforated solids, with circulations through them, that we saw we could do by means of linked gyrostats. But something that we cannot do by linked gyrostats, we can do by the perforated bodies with fluid circulation: we can make a model gas! The mutual action at a distance, repulsive or attractive according to the mutual aspect of the two bodies when passing within collisional distance of one another, suffices to produce the change of direction of motion in collision, which essentially constitutes the foundation of the kinetic theory of gases.

There remains, however, as we have seen before, the difficulty of providing for the case of actual impacts between the solids.

Let us annul the solids, and leave the liquid performing irrotational circulation round vacancy, in the place of the solid cores which we have hitherto supposed; or let us annul the rigidity of the solid cores of the rings, and give them molecular rotation according to Helmholtz's theory of vortex motion. As to whether, however, when the vortex theory of gases is thoroughly worked out, it will or will not be found to fail in a manner analogous to the failure already pointed out in connection with the kinetic theory of gases composed of little elastic solid molecules, one cannot at present speak with certainty.

PROGRESS OF CHEMISTRY SINCE 1848.¹

WITH the death of Berzelius in 1848 ended a well-marked epoch in the history of chemistry: with that of Dumas—and, alas! that of Wurtz also—in 1884 closes a second.

The differences between what may properly be termed the 'Berzelian era,' and that with which the name of Dumas will forever be associated, show themselves in many ways, but in none more markedly than by the distinct views entertained as to the nature of a chemical compound.

According to the older notions, the properties of compounds are essentially governed by the qualitative nature of their constituent atoms, which were supposed to be so arranged as to form a binary system. Under the new ideas, on the other hand, it is mainly the number and arrangement of the atoms within the molecule which regulate the characteristics of the compound, which is to be looked on, not as built up of two constituent groups of atoms, but as forming one group. Another striking difference of view between the chemistry of the Berzelian era and that of

what we sometimes term the 'modern epoch,' is illustrated by the so-called 'substitution theory.' Dumas, to whom we owe this theory, showed that chlorine can take the place of hydrogen in many compounds, and that the resulting body possesses characters similar to the original. But there is another change of view, dating from the commencement of the Dumas epoch, which has exerted an influence, equal, if not superior, to those already named on the progress of chemistry, and that is, as to the use of equivalent or molecular weights.

The theory of organic radicals, developed by Liebig so long ago as 1834, received numerous experimental confirmations in succeeding years. Bunsen's classical research on cacodyl, proving the possibility of the existence of metallo-organic radicals capable of playing the part of a metal, and the isolation of the hydrocarbon ethyl by Frankland in 1849, laid what the supporters of the theory deemed the final stone in the structure.

The fusion of the radical and type theories, chiefly effected by the discovery in 1849 of the compound ammonias by Wurtz, brings us to the dawn of modern chemistry. Henceforward organic compounds were seen to be capable of comparison with simple inorganic bodies, and hydrogen capable of replacement not only by chlorine or by a metal, but by an organic group or radical.

At the Edinburgh meeting of this association in 1850, Williamson read a paper on 'Results of a research on aetherification,' which not only included a satisfactory solution of an interesting and hitherto unexplained problem, but was destined to exert a most important influence on the development of our theoretical views: for he proved, contrary to the then prevailing ideas, that ether contains twice as much carbon as alcohol, and that it is not formed from the latter by a mere separation of the elements of water, but by an exchange of hydrogen for ethyl; and this fact, being in accordance with Avogadro's law of molecular volumes, could only be represented by regarding the molecule of water as containing two atoms of hydrogen to one of oxygen, one of the former being replaced by one of ethyl to form alcohol, and the two of hydrogen by two of ethyl to form ether. Then Williamson introduced the type of water (subsequently adopted by Gerhardt) into organic chemistry, and extended our views of the analogies between alcohols and acids by pointing out that these latter are also referable to the water-type, predicting that bodies bearing the same relations to the ordinary acids as the ethers do to the alcohols must exist,—a prediction shortly afterwards (1852) verified by Gerhardt's discovery of the anhydrides.

Again, in 1852, we note the first germs of a theory which was destined to play an all-important part in the progress of the science; viz., the doctrine of valency, or atomicity; and to Frankland it is that we owe this new departure. But whether we range ourselves with Kekulé, who supports the unalterable character of the valency of each element, or with Frankland, who insists on its variability, it is now clear to most

¹ Abstract of an address to the chemical section of the British association at Montreal, Aug. 28, 1884, by Professor HENRY ENFIELD ROSCOE, Ph.D., LL.D., F.R.S., F.C.S., president of the section.

chemists that the hard and fast lines upon which this theory was supposed to stand cannot be held to be secure.

But however many doubts may have been raised, in special instances, against a thorough application of the law of valency, it cannot be denied that the general relations of the elements which this question of valency has been the means of bringing to light are of the highest importance, and point to the existence of laws of nature of the widest significance; as seen in the periodic law of the elements first foreshadowed by Newlands, but fully developed by Mendelejeff and Lothar Meyer. But this periodic law makes it possible for us to do more: for as the astronomer, by the perturbations of known planets, can predict the existence of hitherto unknown ones, so the chemist, though of course with much less reliable means, has been able to predict with precision the properties, physical and chemical, of certain missing links amongst the elements; such as ekaluminium and ekaboron, then unborn, but which shortly afterwards became well known to us in the flesh as gallium and scandium.

Arising out of Kekulé's theory of the tetrad nature of the carbon atom, came the questions which have caused much debate among chemists: 1°. Are the four combining units of the carbon atom of equal value, or not? and 2°. Is the assumption of a dyad carbon atom, in the so-called non-saturated compounds, justifiable, or not? The answer to the first of these, a favorite view of Kolbe's, is given in the now well-ascertained laws of isomerism; and from the year 1862, when Schorlemmer proved the identity of the hydrides of the alcohol radicals with the so-called radicals themselves, this question may be said to be set at rest.

Passing from this subject, we arrive, by a process of natural selection, at more complicated cases of chemical orientation; that is, given certain compounds which possess the same composition and molecular formulae but varying properties, to find the difference in molecular structure by which such variation of properties is determined. Problems of this nature can now be satisfactorily solved, the number of possible isomers foretold, and this prediction confirmed by experiment.

The discovery of the aniline colors by Perkin, their elaboration by Hofmann; the synthesis of alizarin by Graebe and Liebermann, being the first vegetable coloring-matter which has been artificially obtained; the artificial production of indigo by Baeyer; and, lastly, the preparation by Fischer of kairine, a febrifuge as potent as quinine,—are some of the well-known recent triumphs of modern synthetical chemistry.

In no department of chemistry has the progress made been more important than in that concerned with the accurate determination of the numerical, physical, and chemical constants, upon the exactitude of which every quantitative chemical operation depends. Amongst the most interesting recent additions to our knowledge, made in this department, we may note the classical experiments, in 1880, of

J. W. Mallet on aluminium, and, in the same year, of J. P. Cooke on antimony, and those, in the present year, of Thorpe on titanium.

In referring to the work in spectrum analysis, Professor Roscoe recalled some of the more remarkable conclusions to which the researches of Lockyer, Schuster, Liveing and Dewar, Wüllner, and others, in this direction, have led. In the first place, it is well to bear in mind that a difference of a very marked kind, first distinctly pointed out by Alex. Mitscherlich, is to be observed between the spectrum of an element and that of its compounds, the latter only being seen in cases in which the compound is not dissociated at temperatures necessary to give rise to a glowing gas; second, that these compound spectra (as, for instance, those of the halogen compounds of the alkaline-earth metals) exhibit a certain family likeness, and show signs of systematic variation in the position of the lines, corresponding to changes in the molecular weight of the vibrating system. Still, it cannot be said that as yet definite proof has been given in support of the theory that a causal connection is to be found between the emission spectra of the several elements belonging to allied groups and their atomic weights, or other chemical or physical properties. In certain of the single elements, however, the connection between the spectra and the molecular constitution can be traced. In the case of sulphur, for example, three distinct spectra are known. The first of these, a continuous one, is exhibited at temperatures below 500°, when, as we know from Dumas' experiments, the density of the vapor is three times the normal, showing that at this temperature the molecule consists of six atoms. The second spectrum is seen when the temperature is raised to above 1000°, when, as Deville and Troost have shown, the vapor reaches its normal density; and the molecule of sulphur, as with most other gases, contains two atoms; and this is a band-spectrum, or one characterized by channelled spaces. Together with this band-spectrum, and especially round the negative pole, a spectrum of bright lines is observed. This latter is doubtless due to the vibrations of the single atoms of the dissociated molecule, the existence of traces of a band-spectrum demonstrating the fact, that, in some parts of the discharge, the tension of dissociation is insufficient to prevent the reunion of the atoms to form the molecule.

The most remarkable results obtained by Abney and Festing show that the radical of an organic body is always represented by certain well-marked absorption-bands; differing, however, in position, according as it is linked with hydrogen, a halogen, or with carbon, oxygen, or nitrogen. Indeed, these experimenters go so far as to say that it is highly probable, that, by this delicate mode of analysis, the hypothetical position of any hydrogen which is replaced may be identified; thus pointing out a method of physical orientation, of which, if confirmed by other observers, chemists will not be slow to avail themselves.

One of the noteworthy features of chemical progress is the interest taken by physicists in fundamental

questions of the science, — Sir William Thomson's interesting speculations, founded upon physical phenomena, respecting the probable size of the atom; and Helmholtz's discussion of the relation of electricity and chemical energy; and the theory of the vortex-ring constitution of matter, thrown out by Sir William Thomson, and lately worked out, from a chemical point of view, by J. J. Thomson of Cambridge.

Another branch of chemistry which has recently attracted much experimental attention is that of thermo-chemistry, — a subject upon which, in the future, the foundation of dynamical chemistry must rest, and one which already proclaims the truth of the great principle of the conservation of energy, in all cases of chemical as well as of physical change. But here, although the materials hitherto collected are of very considerable amount and value, the time has not yet arrived for expressing these results in general terms; and we must therefore be content to note progress in special lines, and wait for the expansion into wider areas.

In conclusion, Professor Roscoe spoke of the part English chemists had played in the past, and of the marked difference between the data-gathering German work, and the systematizing of the facts known, which is going on in England. He also referred to what he considered the best method of educating chemists, — by giving them as sound and extensive a foundation in the theory and practice of chemical science as their time and abilities will allow, rather than forcing them prematurely into the preparation of a new series of homologous compounds, or the investigation of some special reaction, or of some possible new coloring-matter, though such work might doubtless lead to publication, — and called attention to the prominence of English industrial chemistry.

THE CORRELATION OF GEOLOGICAL FORMATIONS.¹

THIS address was devoted to a consideration of a few remarkable exceptions to the rule that similarity of faunas and floras in fossiliferous formations throughout the surface of the world implies identity of geological age. Some interesting contributions have been made to this question by the geological survey of India, where Mr. Blanford's experience has been chiefly derived, and by the geologists of Australia and South Africa; and he first noticed a few typical instances, several of them Indian, in which the system of determining the age of various formations by the fauna or flora has led to contradictory results, and then showed where the source of error appears to lie. The famous Pikermi beds of Greece, a few miles east of Athens, contain a vertebrate fauna nearly always quoted as miocene; but they overlie strata with well-proved pliocene marine Mollusca. The Siwalik beds that flank the Himalaya north of Delhi are still

classified as miocene by most European writers, but are regarded as pliocene by the Indian survey, on evidence found by tracing them west and south into Sind. The Gondwana system of central India, a great sequence of fresh-water beds probably of fluvial origin, over 20,000 feet thick, is of unusual interest on account of the extraordinary conflict of paleontological evidence it presents to the observer. Its subdivisions are numerous, and vary in almost every place of occurrence. One (the Tálchir beds) contains rounded boulders chiefly of metamorphic rocks up to six feet across, embedded in fine silt: others are characterized by an intermingling of floras and faunas that give rise to a mass of contradictions; beds with a Triassic fauna overlying others with Rhaetic or Jurassic floras. The Australian coal-measures and their associated beds present even a more remarkable instance of homotaxial perversity, a Jurassic flora being of the same age as a carboniferous marine fauna. Some of these beds (Hawkesbury) again contain transported boulders, which occur once more in the lower members (Ecca beds) of the Karoo formation of interior South Africa. The latter presents a striking likeness to the Gondwana system of India. In both countries, a thick fresh-water formation occupies a large area of the interior of the country, whilst on the coast some marine Jurassic and cretaceous rocks are found; and as in India, so in South Africa, the uppermost inland mesozoic fresh-water beds are capped by volcanic.

Other examples of discrepancies in paleontological evidence might be given, but he would add merely a mention of the single case known to him in which the discordant records are both marine, namely, Barrande's 'colonies' in Bohemia; but here the discordance is much less than in the cases before cited, and moreover Barrande's conclusion is disputed by other observers.

In most of the cases he had named, the conflict is between the evidence of marine and terrestrial organisms. Manifestly one or the other of these leads to erroneous conclusions; and in making choice between the two, most geologists accept evidence of the marine fossils. The reason is not far to seek. So far as he was aware, no case is known where such an anomaly as that displayed in the Gondwanas of India has been detected amongst marine formations of which the sequence was unquestioned. Further, if we compare the distribution of marine with that of terrestrial and fresh-water animals and plants at the present day, we shall find a very striking difference; and it is possible that this difference may afford a clew to the conditions that prevailed in past times.

Wanderers into what they fancy unexplored tracts in paleontology are likely to find Professor Huxley's footprints on the path they are following. In his paper on the Hyperodapedon, he says: "It does not appear to me that there is any necessary relation between the fauna of a given land and that of the seas on its shores. . . . What now happens geographically to provinces in space, is good evidence as to what, in former times, may have happened to provinces in time; and an essentially identical land-

¹ Abstract of an address to the geological section of the British association at Montreal, Aug. 28, 1884, by W. T. BLANFORD, F.R.S., Sec. 7, G.S., F.R.G.S., president of the section.

fauna may have been contemporary with several successive marine faunae. At present our knowledge of the terrestrial faunae of past epochs is so slight that no practical difficulty arises from using, as we do, sea-reckoning for land time. But I think it is highly probable that sooner or later the inhabitants of the land will be found to have a history of their own."

When these words were written, more than twenty-four years ago, scarcely one of the geological details to which Mr. Blanford called attention was known. He need not point out how wonderful a commentary such details have afforded to Professor Huxley's views. But there is, he believed, an additional distinction between land and marine faunas, that requires notice. At the present day the difference between the land-faunas of different parts of the world is so vastly greater than that between the marine faunas, that if both were found fossilized, whilst there would be but little difficulty in recognizing different marine deposits as of like age from their organic remains, terrestrial and fresh-water beds would in all probability be referred to widely differing epochs.

Our present knowledge of the distribution of terrestrial and marine faunas and floras can be only briefly treated. Among mammals and reptiles, the marine forms are generally the most widely diffused. Fishes give better illustration: eighty families are typically marine, and twenty-nine are confined to fresh water; of the first, fifty are universally, or almost universally, distributed; while of the second, only one (Cyprinidae) is found in five of Wallace's regions, and not one is met with in all six. It is impossible to conceive a greater contrast. The distribution of land and sea Mollusca leads to a similar conclusion as to the relatively narrow range of the land forms. Throughout the marine invertebrata, but few generic types are restricted to particular seas: the majority are found in suitable habitats over a large portion of the oceans. Indeed, the marine provinces that have been hitherto distinguished are founded rather on specific than on generic distinctions. Botany offers a still more remarkable example: so uniform is the marine vegetation of the world, that no separate regions can be established in the ocean, while Drude makes fourteen on the land.

Mr. Blanford alluded to the evidence of the existence of land-regions in past times. Proofs are already accumulated of differences between the fauna of distant countries in tertiary times. The eocene, miocene, and pliocene Vertebrata of North America differ quite as much from those of Europe as do the genera of the present day; and there was as much distinction between the mammalia of the Himalayas and of Greece when the Siwalik and Pikermi faunas were living as there is now. The reptiles of the American Jurassic deposits present wide differences from those of the European beds of that age. But there is no reason for supposing that the limits or relations of the zoölogical and botanical regions in past times were the same as they now are. It is quite certain, indeed, that the distribution of land-areas has undergone enormous variations, whether the great oceanic tract

has remained unchanged in its general outlines or not; and the migration of the terrestrial fauna and flora must have been dependent upon the presence or absence of land communication between different continental tracts: in other words, the terrestrial regions of past epochs, although just as clearly marked as those of the present day, were very differently distributed.

The idea that marine and terrestrial faunas and floras were similar throughout the world's surface in past times, is so ingrained in paleontological science, that it will require many years yet before the fallacy of the assumption is generally admitted. No circumstance has contributed more widely to the belief than the supposed universal diffusion of the carboniferous flora. The evidence that the plants which prevailed in the coal-measures of Europe were replaced by totally different forms in Australia, despite the closest similarity in the marine inhabitants of the two areas at the period, will probably go far to give the death-blow to an hypothesis that rests upon no solid ground of observation. In a vast number of instances it has been assumed that similarity between fossil terrestrial faunas and floras proves identity of geological age; and by arguing in a vicious circle, the occurrence of similar types, assumed without sufficient proof to belong to the same geological period, has been alleged as evidence of the existence of similar forms in distant countries at the same time.

It may perhaps have surprised some, that Mr. Blanford scarcely alluded to any American formations, and especially that he had not mentioned so well-known and interesting a case of conflicting paleontological evidence as that of the Laramie group. His reason was simply, that there were probably many present who were personally acquainted with the geology of the American cretaceous and tertiary beds, and who were far better able to judge than he of the evidence as a whole. To all who are studying such questions in America, he thought it would be more useful to give the details of similar geological puzzles from the eastern hemisphere, than to attempt an imperfect analysis of difficult problems in the great western continent.

THE PHYSIOLOGY OF DEEP-SEA LIFE.¹

THE physiology of the deep-sea life has, until lately, received but little attention from professed physiologists. No one has yet set forth the numerous difficulties which are encountered, when the attempt is made to comprehend the mode in which the ordinary physiological processes of Vertebrata and other animals are carried on under the peculiar physical conditions which exist at great depths.

A knowledge of the conditions under which gases occur in a state of absorption in the ocean-waters

¹ Abstract of an address to the biological section of the British association at Montreal, Aug. 28, 1884, by H. N. MOSELEY, Esq., M.A., F.R.S., Linacre professor of human and comparative anatomy in the University of Oxford, president of the section.

is of primary importance to the physiologist. It appears from the results of Professor Dittmar's researches into the composition of the ocean-water collected by the Challenger, that, contrary to what was before suspected, the presence of free carbonic acid in sea-water is an exception. Hence, with regard to Mr. Murray's interesting discovery, that, after certain depths are reached, pteropod shells are dissolved, and disappear from the sea-bottom, and at certain farther depths Globigerina shells suffer the same fate, Professor Dittmar holds that the solution is not due to the presence of free acid, but to the solvent action of the sea-water itself. Thus the amount of carbonic acid normally present throughout the ocean cannot be inimical to life; but there must be in the depths of the ocean numerous bodies of richly carbonated water.

French physiologists have lately commenced researches on some of the problems of deep-sea life. Experiments have been made by Mr. Regnard with a view of determining the effects of high pressures, corresponding with those of the deep sea, on various organisms. Yeast, after being exposed to a pressure of a thousand atmospheres, equal to a depth of about sixty-five hundred fathoms of sea-water, for an hour, was mixed with a solution of sugar. An hour elapsed before any signs of fermentation appeared; and a mixture of yeast and sugar solution did not ferment at all whilst under a pressure of six hundred atmospheres, equal to a depth of about thirty-nine hundred fathoms. Algae, seeds of phanerogamic plants, infusoria, and even Mollusca and leeches, were found to be thrown into a condition of sleep, or latency, by exposure to similar pressures, recovering from this condition after a shorter or longer period of return to normal conditions. A fish without a swimming-bladder, or one with the bladder emptied of air, may be submitted to a pressure of a hundred atmospheres, equivalent to a depth of six hundred and fifty fathoms, without injurious effect. At two hundred atmospheres, equivalent to a depth of thirteen hundred fathoms, it becomes torpid, but soon revives when the pressure is removed. At three hundred atmospheres, equivalent to a depth of about two thousand fathoms, the fish dies. These experiments are of the highest interest. The pressure made use of was obtained by means of water, in the absence of air other than that absorbed at the normal atmosphere pressure; and thus the physical conditions produced were closely similar to those actually existent in the deep sea. They are the first of their kind.

Professor Paul Bert's somewhat similar experiments related to a different question altogether; namely, the effect, on aquatic organisms, of water subjected to the pressure of compressed air. He found that young eels were rapidly killed when subjected to a pressure of only fifteen atmospheres, and could not survive one of even seven atmospheres for any considerable time. He pointed out the essential difference between the conditions produced in such experiments and those existing in the deep sea, where the charge of oxygen contained by the water has been

taken up at the surface under a pressure of one atmosphere only.

A question of the utmost moment, and one that has received a good deal of attention, is that as to the source of food of the deep-sea animals. Certainly a large proportion of this food is derived from the life on the ocean-surface. The *débris* of pelagic animals sinks slowly downwards, forming on its passage a sparsely scattered supply of food for any animals possibly living at intermediate depths, but becoming concentrated, as it were, on the bottom. A large part of the food-supply is also derived from the *débris* of the coasts, either directly from the littoral zone, or by rivers and the action of the tides from terrestrial life. Deep-sea life appears to diminish in abundance as coasts are receded from. Unfortunately, our knowledge of pelagic vegetable life is very imperfect, and it is to be hoped that botanists may be led to take up the subject. It will then be possible to form a nearer estimate of the extent to which plants are capable of forming a sufficient ultimate food-source for the greater part of the pelagic fauna, and, through it, of deep-sea life. The question is of importance; because, if the deep sea derived its main supply from the coasts and land-surfaces in the early history of the habitation of the globe by animals, there can have existed scarcely any deep-sea fauna until the littoral and terrestrial faunas and floras had become well established. It seems certain that the food, as it reaches the deep sea, is mostly in the form of dead matter; and it may be that the long but slender backward-directed teeth of many deep sea fish, resembling those of snakes, are used rather as aids for swallowing whole other fishes which have fallen from above, dead, and thus making the best of an occasional opportunity of a meal, than for catching and killing living prey.

Many interesting results may be expected when the histology of animals from great depths comes to be worked out, and especially that of the special sense-organs. At present very little has been attempted in this direction; principally, no doubt, because deep-sea specimens are too precious to be used for the purpose. With regard to the all-important question of the nature of the light undoubtedly present in the deep sea, it is hardly possible to accept Professor Verrill's recent startling suggestion (*Science*, iv. 8), that sunlight penetrates to the greatest depths with perhaps an intensity at from two thousand to three thousand fathoms, equal to that of some of our partially moonlight nights. Such a conjecture is entirely at variance with the results of all experiments on the penetration of sea-water by sunlight, as yet made by physicists,—results which have prevented other naturalists from adopting this solution of the problem.

The progress of research confirms the conclusions, early formed, that it is impossible to determine any successive zones of depth in the deep-sea regions, characterized by the presence of special groups of animals. Some groups of animals appear to be characteristic of water of considerable depth; but representatives of them struggle up into much shallower regions. There are numerous genera, and even spe-

cies, which range even from the shore-region to great depths. These facts add seriously to the difficulties encountered in the attempt to determine approximately the depths at which geological deposits have been found. Dr. Theodore Fuchs has attempted to determine what geological strata should be considered as of deep-sea formation; but, as he defines the deep-sea fauna as commencing at a hundred fathoms, and extending downwards to all depths, his results have little value as indicating the depths of ancient seas, or the extent of upheaval or depression of their bottoms. Mr. John Murray has shown that the depths at which modern deep-sea deposits have been formed can be approximately ascertained by the examination of their microscopical composition, and the condition of preservation of the shells and spicules.

The most important question with regard to life in the ocean, at present insufficiently answered, is that as to the conditions with regard to life of the intermediate waters between the surface and the bottom. The greatest uncertainty and difference of opinion exist as to whether the intermediate waters are inhabited at all by animals, and, if they are inhabited, to what extent; and these intermediate waters constitute by far the greater part of the ocean. Great care should be exercised in drawing conclusions from the depths ascribed to animals in some of the memoirs in the official work on the Challenger expedition. In many instances it is quite possible that a particular specimen may have entered the net at any depth.

With regard to the constitution of the deep-sea fauna, one of its most remarkable features is the general absence from it of paleozoic forms, excepting so far as representatives of the Mollusca and Brachiopoda are concerned; and it is remarkable, that, amongst the deep-sea Mollusca, no representatives of the Nautilidae and Ammonitidae, so excessively abundant in ancient periods, occur, and that *Lingula*, the most ancient brachiopod, should occur in shallow water only. It might well have been expected, that, had the deep sea been fully colonized in the paleozoic period, a considerable series of representative forms of that age might have survived there, in the absence of most of the active physical agents of modification which characterize the coast-regions.

With regard to the origin of the deep-sea fauna, there can be little doubt that it has been derived almost entirely from the littoral fauna, which also must have preceded, and possibly given rise to, the entire terrestrial fauna; yet it is not improbable that we should look to the pelagic conditions of existence as those under which most of the earliest types of animal life were developed. Nearly all the present inhabitants of the littoral zone revert to the pelagic free-swimming form of existence in their early developmental stages. And these pelagic larval forms are in many cases so closely alike in essential structure, though springing from parents widely differentiated from one another in the adult form, that it is impossible to regard them as otherwise than ancestral. The various early pelagic free-swimming forms, represented now mostly only by larvae, gradually adapted

themselves to coast-life, and underwent various modifications to enable them to withstand the beating of the surf on the shores, and the actual modifying alterations of the tides, which, together with other circumstances of coast-life, acted as strong impulses to their further development and differentiation. Some developed hard shells and skeletons as protections; others secured their position by boring in the rocks or mud; others assumed an attached condition, and thus resisted the wash of the waves.

It is because the ancestors of nearly all animals have passed through a littoral phase of existence, preceded mostly by a pelagic phase, that the investigations now being carried on, on the coasts in marine laboratories, throw floods of light on all the fundamental problems of zoölogy. From the littoral fauna a gradual migration must have taken place into the deep sea; but probably this did not occur till the littoral fauna was very fully established, and considerable pressure was brought to bear on it by the struggle for existence. Life, too, must have become abundant in the littoral zone before there could have been a sufficient food-supply in the deeper regions adjoining it. Not until the development of terrestrial vegetation and animal life can the supply have reached its present abundance. Such a condition was, however, certainly reached in the carboniferous period. From the general absence of representatives of paleozoic forms from the deep sea, it is just possible, that, if deep oceans existed in paleozoic periods, they may not have been colonized at all, and that active migration into deep waters commenced in the secondary period. Very possibly the discharges of carbonic acid from the interior of the earth, which Professor Dittmar believes may have been sufficient to account for the vast existing deposits of coal and limestone, may have been much more abundant over the deep-sea beds in the paleozoic period, than at present, and have rendered the deep waters more or less uninhabitable.

RECENT GEOGRAPHICAL DISCOVERY.¹

AFTER some introductory remarks referring to his previous visit to Canada, Gen. Lefroy alluded to the relations of geography to geology as instanced in the changes in the earth's surface within historical times by the operation of geological causes. A recent German writer, Dr. Hahn, has enumerated ninety-six more or less extensive tracts known to be rising or sinking. For example: Mr. R. A. Peacock has accumulated evidence that the Island of Jersey had no existence in Ptolemy's time, and Mr. A. Howarth has collected similar proofs with regard to the arctic regions; and every fresh discovery, notably those of the gallant and ill-fated DeLong and of Nordenskiöld, adds to the number. Professor Hull has reached the conclusion that the land between Suez and the

¹ Address to the geographical section of the British association at Montreal, Aug. 28, 1884, by Gen. Sir J. H. LEFROY, R.A., C.B., K.C.M.G., F.R.S., F.S.A., vice-president of the Royal geographical society, president of the section.

Bitter Lakes has risen since the exodus; and from the Indian survey it is 'almost certain' that the mean sea-level at Madras is a foot lower than it was sixty years ago. From the Chinese annals it is learned that the so-called Hot Lake (Myk-kul) of Turkestan was formed about a hundred and sixty years ago; and there seems no good reason to reject the Japanese legend that Fusi-yama itself was thrown up in the third century before our era.

He then touched lightly on the progress made in our knowledge of the geography of the Dominion of Canada, which comprises within its limits the pole of vertical magnetic attraction, commonly called the magnetic pole, and the focus of greatest magnetic force, also often but incorrectly called a pole. The first of these, discovered by Ross in 1835, was revisited in May, 1847, by officers of the Franklin expedition, whose observations have perished, and was again reached, or very nearly, by McClintock in 1859, and by Schwatka in 1879. Neither of these explorers, however, was equipped for observation. The utmost interest attaches to the question whether the magnetic pole has shifted its position in fifty years; and, although far from rating the difficulty lightly, it is probably approachable overland, without the great cost of an arctic expedition. The second, which is in the neighborhood of Cat Lake, has never been visited, although Dr. R. Bell was within two hundred miles of it. These two objects, and the exploration of an almost unknown tract of some seventy thousand square miles, lying east of Athabasca River, were declared worthy of the scientific ambition and energy of the dominion. He alluded also to the extent and importance of Lake Misstassini, which has recently been discovered in no very remote part of the dominion, — a lake rivalling Lake Ontario, if not Lake Superior, in magnitude.

He then mentioned the report of Lieut. R. P. Rodgers, U. S. N., on the state of the canal-works at Panama so lately as Jan. 25 last, and read the official returns of the amount of excavation during the months of October, 1883–March, 1884, by which it appeared that the quantity excavated per month had greatly increased during that time, and shows that the limit has not been reached. The two great problems which await solution are, how to deal with the River Chagres, and how to manage a cutting nearly four hundred feet deep. The Chagres, which is subject to great fluctuations of volume, it is proposed to arrest by an enormous dike, 1,050 yards long at the bottom, 2,110 yards at the top, 110 yards thick at the base, and 147 feet in the greatest height; the overflow of the reservoir so constructed to be led away by two artificial channels, partly utilizing the old bed. The cutting, nearly five hundred feet wide at the top, it is proposed to attack by gangs working on twelve different levels at the same time, one each side of the summit, dividing the width at each level into five parallel sections. Thus there will be a hundred and twenty gangs at work together, and it is confidently hoped that the whole will be really finished in 1888.

He next turned to another quarter, and referred to

the mission intrusted to Mr. Joseph Thomson last year, in East Africa, by the Royal geographical society. After an unsuccessful start from Zanzibar in March of last year, — in which, however, he reached Kilima-njaro, and ascended it about nine thousand feet, — he returned to the coast from Taveta, and started again in July, this time from Mombasa. We are not yet fully acquainted with his route; but we know that he again reached the great mountain reputed to have an elevation of more than twenty thousand feet; that thence he reached the east side of Lake Nyanza; that he is the first who has stood on the shores of Lake M'Baringo; that thence, always among natives who had never before seen a white man, he reached Mount Kenia, reputed to be eighteen thousand feet high, and found his way back to the coast without any conflict or loss of life by violence; and this after a journey of about five hundred miles, nearly the whole of it through a country previously unknown to geography. Before Mr. Thomson's return to Zanzibar, Mr. H. H. Johnston, under direction of a committee of this association, whose plans are devoted primarily to the investigation of the fauna and flora of Kilima-njaro, had left Zanzibar in good health, and with every hope of ultimate success.

The president then alluded to the unfortunate French expedition of Col. Flatters, who, together with several other officers and men, was killed by the Youaregs in February, 1881. The French travellers have emphasized the probable consequences of the rapid progress of the religion of Mohammed among the African races of the northern equatorial zone, which in time may reach the populous basin of the Kongo, and may greatly affect the white settlements and missionary enterprises in Central Africa hereafter.

The Upper Kongo, from Stanley Falls to Stanley Pool, is now pretty well known; but as to its tributaries, much remains to be learned. Mr. Stanley has discovered two new lakes. The labors of that energetic traveller, Mr. de Brazza, have to a great extent cleared up the geography of the region included between the Kongo and the Ogowé, from the equator southwards; and there are now said to be twenty-two trading-stations in this part of the country. We are not informed what commerce exists. Higher up, but still to the north, Mr. Stanley has ascended the Aruwimi about a hundred miles, without having solved a question of no little interest; namely, whether it is identical with the Wellé, and takes its rise in the same watershed which feeds the White Nile, or whether we have not, beyond its sources, a drainage system, as yet untraced, but which may connect together a number of rivers whose relations to one another, and whose final outlet, are alike unknown. Lupton Bey reported, nearly two years ago, that a very large lake had been visited by one of his native subordinates west of the Aruwimi; and it is, in his opinion, probable that the Wellé flows into it. The southern basin of the Kongo has been crossed from Koando to Nyan-givé by the late Dr. Pogge and Lieut. Wissman, the latter of whom continued his journey by Yabora to Zanzibar. He brings confirmation of the often re-

ported existence of a dwarfish race on the upper waters of the Sankuru.

Proceeding southward to the region of Portuguese exploration, Messrs. Britto, Capello, and Ivens, who reached the Upper Quango in 1878, returned last January to Loando, with the intention, it is said, of descending one of its great tributaries. They are now on the Kumene. Dr. Pogge compares the climate of Mussumba on the 8th parallel, in the month of December, to that of North Germany; and the fact illustrates, what we learn from so many other quarters, that much of the interior of Africa belongs, by reason of its elevation above the sea, to a far more temperate zone, and is better suited to the European constitution, than its geographical position promises. The terrible prevalence of fever, which has cost so many lives, will probably be mitigated in time, and by improved accommodation. The hills are comparatively free from it. The progress already made in the white occupation of Central Africa was well shown by a table of actual centres of trade, or missionary institutions (a hundred and twenty in number), now established there.¹

He then took up the Russian project for diverting the Oxus, or Amu Darya, from the Sea of Aral to the Caspian; the level of which, according to Mr. George Kennan, a recent American traveller, is steadily but slowly falling, notwithstanding the enormous quantity of water poured in by the Volga, the Ural, and other rivers. In fact, Col. Vinukof says, that the Caspian is drying up fast, and that the fresh-water seals, which form so curious a feature of its fauna, are fast diminishing in number. At first view, there would not appear great difficulty in restoring water communication, the point where the river would be diverted being about two hundred and sixteen feet above the Caspian; but accurate levelling has shown considerable depressions in the intervening tract. Certainly the Oxus, or a branch of it, once flowed into the Caspian Sea. Prof. R. Lentz, of the Russian Académie impériale des sciences, sums up his investigation of ancient authorities by affirming that there is no satisfactory evidence of its ever having done so before the year 1320. Passages which have been quoted from Arab writers of the ninth century, only prove, in his opinion, that they did not discriminate between the Caspian Sea and the Sea of Aral. There is evidence that in the thirteenth and fourteenth centuries the river bifurcated, and one branch found its way to the Caspian, but probably ceased to do so in the sixteenth century. This agrees with Turkoman traditions. We may safely conclude that the thing will not be done; nor is it at all probable that Russian finances will permit the alternative proposal of cutting a purely artificial canal by the shortest line, at an estimated expense of from fifteen to twenty million roubles.

One of the finest feats of mountaineering on record was performed last year by Mr. W. W. Graham, who reached an elevation of twenty-three thousand five hundred feet in the Himalayas about twenty-nine hun-

dred feet above the summit of Chimborazo, whose ascent by Mr. Whympfer, in 1880, marked an epoch in these exploits. Mr. Graham was accompanied by an officer of the Swiss army, an experienced mountaineer, and by a professional Swiss guide. They ascended Kabru, a mountain visible from Darjeeling, lying to the west of Kanchinjunga, whose summit still defies the strength of man.

The primary triangulation of India, commenced in the year 1800, is practically complete. Much secondary triangulation remains to be executed, but chiefly outside the limits of India proper. The Pisgah views, by which some of the loftiest mountains in the world have been fixed in position, sometimes from points in the nearest Himalayas, a hundred and twenty miles distant, only serve to arouse a warmer desire for unrestrained access. The belief, long entertained, that a summit loftier than Mount Everest exists in Thibet, is by no means extinct; but it is possible that the snowy peak intended may prove eventually to be the Mount Everest itself of the original survey.

The Upper Oxus has now been traced from its sources in the Panjah, chiefly by native explorers; and to them we may be said to be indebted for all we know of Nepaul, from which Europeans are as jealously excluded as they are from the wildest central Asian kanate, although Nepaul is not so far from Calcutta as Kingston is from Quebec.

The Australian continent has been crossed again from east to west by Mr. Mills, who started with thirty camels attended by five Afghan drivers. Six of them died from the effects, as was supposed, of eating poisonous herbage. Mr. Mills did not deviate much from the tracks of the late Mr. W. C. Gosse and of Mr. J. Forrest: his journey has therefore added little to previous geographical knowledge; but it has helped to make the route better known, and afforded fresh evidence that the value of the camel in those terrible Australian saharas is in no degree less than it is where he has long been known as the 'ship of the desert.' Another traveller, Mr. C. Winnecke, starting from the Cowarie station on the Warburton River, in 28° south, has traversed about four hundred miles of new country in a northerly direction, and made a sketch-map of forty thousand square miles, up to Goyders Pillars, — a remarkable natural feature in the Tarleton Range. He, too, owed his success to the employment of camels. The international circumpolar expeditions have added, perhaps, to local knowledge, especially as regards the climate and means of supporting life at various stations, but not much, so far as reported, to geography generally. To this remark, however, a brilliant exception must be made. The distinction of the nearest approach to the north pole, yet made by man, has been won by the late Lieut. Lockwood and Sergt. Brainerd, of Lieut. Greely's expedition. They reached on May 13, 1882, an island not before known, in latitude 83° 24' north, longitude 44° 5' west, now named after its discoverer. This is four or five miles beyond Capt. Markham's farthest point (83° 20' north), and it appears to be by no means the only geographical achievement which in some measure

¹ A list of these is given in an appendix to the address, with their geographical positions collated with great care.

rewards the painful sufferings and losses of the party. Lieut. P. H. Ray, U.S.A., has also rectified many details of the map about Point Barrow, and discovered a range of hills, which he has named the Meade Mountains, running east from Cape Lisburne, from which at least two streams, unmarked, flow into the Polar Sea. We may expect similar service from the Italian parties at Patagonia, and from the Germans in South Georgia. Since the voyage of the Challenger, no marine researches have been more fruitful of results than those of the *Talisman* and the *Dacia*. The first was employed last year by the French government, to examine the Atlantic coasts from Rochefort to Senegal, and to investigate the hydrography and natural history of the Cape Verde, Canary, and Azores archipelagoes. The other ship, with her companion the *International*, was a private adventure, with the commercial purpose of ascertaining the best line for a submarine telegraph from Spain to the Canaries. These last two made some five hundred and fifty soundings, and discovered three shoals, one of them with less than fifty fathoms of water over it, between the continent of Africa and the islands. If we draw a circle passing through Cape Mogador, Teneriffe, and Funchal, its centre will mark very nearly this submarine elevation: the other two lie to the north of it. The *Talisman* found in mid-ocean but sixteen hundred and forty fathoms, among soundings previously set down as over two thousand fathoms.

Gen. Lefroy then spoke of the extension of railways in Mexico, South America, Africa, and Asia, and of the agreement to refer local time on this continent to a succession of first meridians, one hour apart. The next step will not be long delayed: that is, the agreement of the civilized world to use one first meridian; Paris, Ferrol, Washington, Rio de Janeiro, gracefully, as we venture to hope, giving that precedence to Greenwich, which is demanded by the fact that an overwhelming proportion of the existing nautical charts of all nations, and of maps and atlases in most of them, already refer their longitudes to that meridian. No other change would be so easy, or so little felt.

THE GENERAL STATISTICS OF THE BRITISH EMPIRE.¹

WE will group our statistics under the following headings: 1°. The area consisting of widely extended regions; 2°. The inhabitants of these many lands; 3°. The works of man as they are displayed in this vast theatre of action.

First, then, the area of the British Empire may be set down at more than eight and a half millions of square miles. Out of this total, there are only a hundred and twenty thousand square miles in the United Kingdom. There are a million and a half of square

miles in India; and the remainder, or seven millions, belong to the colonies and to the scattered possessions.

But there are other regions which have fallen, or are falling, under its political control more or less, such as Egypt, including a part of the Egyptian Sudan, some districts in southern Arabia, a part of Borneo, Zululand, the Transvaal, Afghanistan, and Beluchistan. Thus the total area, directly or indirectly, under the authority of the British empire, may be taken at nearly ten millions of square miles, or about one-fifth of the fifty millions of square miles composing the habitable globe.

As might be expected in an empire whereof the real basis of power is maritime, the coast-line is of an extraordinary length, to be measured by about 28,500 miles, with forty-eight large harbors. For the whole of this length, marine surveys have been prepared. But greatness does not depend on area alone, and there is a vast range in the scale of value for lands. Out of the ten millions of square miles, hardly one-fifth is cultivated or occupied, in the widest use of the term 'occupation.' In India, which is popularly, though not quite correctly, supposed to be thickly populated, the cultivable waste is not less than a quarter of a million of square miles.

In the second place, respecting the inhabitants, the total population amounts to 305,000,000 of souls in those regions which are included directly in the empire. This mass of humanity is composed of many diverse nationalities, a cardinal distinction between which is that of religion. Christianity, the religion of the dominant race, is professed by one-seventh of the whole. The religion which includes the largest number is Hinduism. There are 188,000,000 of Hindus; and it may, indeed, be said that the whole Hindu race is subject to the British crown. The Hindus, then, form more than a half of the total population in the empire. The number of Buddhists is not considerable, amounting to about 7,000,000. The imperial area is, on the whole, but sparsely populated, with an average of only thirty-three persons to the square mile, notwithstanding the mighty aggregate of the people, as the population is most unequally distributed.

The third and last heading relates to the works of man, his riches and power, his industrial and commercial operations.

One, among the primary tests of national resources, is the public revenue. The total of yearly revenue and receipts, governmental and local, amounting to £264,000,000 sterling, is unequalled, but falls at the moderate rate of one and a quarter pounds sterling per head of the total of British subjects. There is a large revenue received throughout the empire for local purposes. This income (including various receipts, but excluding loans) amounts to hardly less than £61,000,000 sterling yearly; and the greater part is levied by direct taxation.

Another test of power relates to the provision for external defence and internal protection. Now the men trained to arms in the British empire may be stated at 850,000, including the regular British forces

¹ Abstract of an address to the economic science and statistics section of the British association at Montreal, Aug. 28, 1884, by Sir RICHARD TEMPLE, Bart., G.C.S.I., C.I.E., D.C.L., LL.D., F.R.G.S., president of the section.

at home and abroad, the militia, and volunteers in the United Kingdom, and in the colonies, the British native forces in India, and other countries. This includes 10,000 Egyptian troops under a British general, but excludes the forces of the native states of India, and of the other countries politically connected with the empire. If, however, the forces of the native states of India were to be added (and they are generally available for imperial purposes), then the total of 850,000 would be raised to nearly 1,000,000. Thus the men under arms, or effectively trained to arms, are in number more than 750,000, and, under the last-named computation, would amount to nearly 1,000,000. The defensive armaments of the empire, by sea and land, cost £41,000,000 sterling annually, or twenty per cent of the total of revenue and receipts. The police for the empire numbers 560,000. Thus we have, for the whole empire, an average of one policeman to every 571 of the people, and to every sixteen square miles.

It is never to be forgotten, that one of the main reasons why the British empire is able to keep its land-forces at a comparatively low scale is its preponderance at sea. The predominance which we hope to find in the British navy will hardly be shown by the enumeration of ships. With this caution, however, it may be stated that there are 246 British war-vessels afloat, or in commission; of which 72 are sailing-ships, and 174 have steam-power. There are now 63 iron-clads, either complete or nearly complete. The number of officers and men amounts to 57,000. The number of iron-clads ready for action at the shortest notice is now 44, of which 25 are at sea.

The mercantile marine has nearly half of the steam tonnage, of the carrying power of the port of entries, and of the freight earnings of all the nations together, and two-thirds of the ship-building. The total trade of the British empire cannot be easily exhibited statistically. However, if the aliquot parts of the trade of the principal nations be computed, then 34 per cent, or one-third of the world's commerce, pertains to the British empire.

The manufactures of the United Kingdom are valued at £818,000,000 sterling annually. In general terms, it may be stated, that British manufactures form one-third of those for all Europe put together. The great competitor is of course the United States, where the value appears to exceed that of the United Kingdom. The American manufactures are indeed wonderful, not only in their present magnitude, but in the rapidity of their progress, and in the prospect of their extension.

It follows from these facts, that the wealth of the United Kingdom in land, cattle, railways and public works, houses and furniture, merchandise, bullion, shipping, and sundries, valued at £8,720,000,000 sterling, exceeds that of any European state, and is just double that of Russia. But it is exceeded by the corresponding figure for the United States, namely, £9,495,000,000 sterling. The £8,720,000,000 of British wealth represent a sum seven times the annual income, namely, £1,247,000,000, which seems to be a

fair calculation. According to this, the British people earn 14 per cent on their capital, which rate is about the same as that of the United States. It exceeds the corresponding ratio on the continent of Europe. But it is considerably surpassed by the ratios in Canada and Australia, — 18 and 22 per cent respectively.

The construction of public works is a test of national progress. Those works which may here be selected for mention are railways, electric telegraphs, and canals. It is calculated that 46 per cent of the railway traffic of the world is done by the railways of the British Empire: the distances run, however, are less than on the continent of Europe or in the United States. The electric telegraph does six times as much in the old country as in the new.

The total public debt, governmental and municipal, for the British Empire, reaches a total of £1,312,000,000 sterling.

He concluded this statistical summary by adverting to a group of subjects into which moral considerations largely enter; namely, thrift and education.

The decrease of crime and pauperism is satisfactory in the United Kingdom; while pauperism hardly exists in the other dominions of the empire, and the charitable funds raised in the United Kingdom are enormous. The number of patients in the hospitals, though large, is not remarkable relatively to the size of the empire.

Respecting education, there are 5,250,000 pupils at schools in the United Kingdom, 860,000 in Canada, 611,000 in Australia, and 2,200,000 in India, making up a total of 8,921,000 pupils in the British Empire. The fact is, that in India, although education has made a remarkable progress within the last generation, yet the lee-way to be made up was enormous, owing to the neglect of many centuries; and many children of a school-going age still remain out of school. But the comparison attains special interest when made with the United States, where a truly noble progress is exhibited, and where the number of pupils reaches to 10,000,000, the annual expenditure being £17,000,000 sterling. Thus the extraordinary fact remains, that in respect of educational statistics the United States are numerically in advance of even the British Empire.

The religious missions to non-Christian nationalities constitute a bright feature in the British Empire. The statistics of the Roman Catholic missions are not fully known, but their operations are very considerable. The income of the various Protestant missionary societies is hardly less than £750,000 sterling annually, and the number of European ordained missionaries maintained by them is about 900.

ON THE RELATION OF MECHANICAL SCIENCE TO OTHER SCIENCES.¹

THERE are those who object that section G deals too little with pure science, too much with its applica-

¹ Abstract of an address to the mechanical science section of the British association at Montreal, Aug. 28, 1884, by Sir F. J. BRAMWELL, F.R.S., V.P.Inst.C.E., president of the section.

tions. It may be, as the members of section G might retort, that it is possible to attend so much to pure science as to get into the unchecked region of scientific speculation, and that, had the members of section G been debarred from the application of science, the speculation of Dr. Lardner might to the present day have been accepted as fact.

The speaker thought all men, even though they be followers of science in its purest and most abstract form, must concur in the propriety of section G dealing with engineering subjects generally, as well as with abstract mechanical science. This admitted he would ask—certain what the answer must be—whether there is any body of men who more appreciate and make greater use of the applications of pure science than do the members of this section. Surely every one must agree that the engineers are those who make the greatest practical use, not only of the science of mechanics, but of the researches and discoveries of the members of the other sections of this association.

It would be the purpose of his address to establish the proposition, that not only is section G the section of mechanical science, but it is emphatically the section, of all others, that applies in engineering, to the uses of man, the sciences appertaining to the other sections of the association,—an application most important in the progress of the world, and an application not to be lightly regarded even by the strictest votaries of pure science; for it would be in vain to hope that pure science would continue to be pursued, if from time to time its discoveries were not brought into practical use. The connection between this section and that of mathematical and physical science (A) is most intimate. Without a knowledge of thermal laws, the engineer engaged in the construction of heat-motors will find himself groping in the dark. He anticipated, from the application of thermal science to practical engineering, that great results are before us in those heat-motors, such as the gas-engine, where the heat is developed in the engine itself. Passing from heat-motors, and considering heat as applied to metallurgy: from the time of the hot blast to the regenerative furnace, it is due to the application of science by the engineer that the economy of the hot blast was originated, and that it has been developed by the labors of Lowthian, Bell, Cowper, and Cochrane. Equally due to this application are the results obtained in the regenerative furnace, in the dust-furnace of Crampton, and in the employment of liquid fuel, and also in operations connected with the rarer metals, the oxygen-furnace and the atmospheric gas-furnace, and, in its incipient stage, the electrical furnace. To a right knowledge of the laws of heat, and to their application by the engineer, must be attributed the success that has attended the air-refrigerating machines, by the aid of which fresh meat is at the end of a long voyage delivered in a perfect condition; and to this application we owe the economic distillation of sea-water by repeated ebullitions and condensations at successively decreasing temperatures.

Coming to the mathematical side of section A,

whether we consider the naval architect preparing his design of a vessel to cleave the waves with the least resistance at the highest speed, or whether we consider the unparalleled series of experiments of that most able associate of naval architects, the late William Froude, carried out as they were by means of models which were admirable in their material, their mode of manufacture with absolute accuracy to the desired shape, and their mode of traction and of record, we must see that both architect and experimenter should be able to apply mathematical science to their work, and that it is in the highest degree desirable that they should possess, as Froude did, those most excellent gifts, science and practical knowledge.

Passing from section A to section B (chemical science), the preparation from the ore of the various metals is, in truth, a branch of engineering; but to enable this to be accomplished with certainty and economy, it is essential that the engineer and the chemist should either be combined in one and the same person, or go hand in hand.

Reverting to the water-engineer, the chemist and the microscopist have their sciences applied to ascertain the purity of the intended source; and, as in the case of Clarke's beautiful process, by the application of chemistry, water, owing its hardness to that common cause, carbonate of lime, is rendered as soft as the water from the mountain lake.

With regard to the subjects treated by section C (geology), the speaker instanced the Channel tunnel as a case in which, without the aid of geology, the engineer would not be able to give an opinion on the feasibility of the enterprise. The engineers said there is a material, the compact non-water bearing gray chalk, which we have at a convenient depth on the English side, and is of all materials the most suitable. If that exist the whole way across, success is certain. Then came geological science, and that told the engineer that in France the same material existed; that it existed in the same position in relation to other stratifications as it existed in England; that the line of outcrop of the gault lying below it had been checked across; and that, taken together, these indications enabled a confident opinion to be expressed that it was all but certain this gray chalk stratification did prevail from side to side.

To come to section D (biology), the botanical side of it is interesting to the engineer as instructing him in the locality and quality of the various woods that he occasionally uses in his work. With regard to that most important part of the work of D, which relates to 'germs' and their influence upon health, the engineer deals with it thus far: he bears in mind that the water-supply must be pure, and that the building must be ventilated, and that excreta must be removed without causing contamination.

In conclusion, reference was made to the relations of the engineer to the geographical explorer and the student of economic science. The great works, the results of engineering skill, enable the geographer to reach his field of exploration the more readily, and are called into existence by the dictates of the economist.

SOME AMERICAN ASPECTS OF
ANTHROPOLOGY.¹

THE term 'prehistoric' stretches back from times just outside the range of written history into the remotest ages where human relics justify the opinion that man existed. Far back in these prehistoric periods, the problem of quaternary man turns on the presence of his rude stone implements in the drift-gravels and in caves, associated with the remains of what may be called the mammoth fauna. Not to recapitulate details, the point to be insisted on is, how the effect of a quarter of a century's research and criticism has been to give quaternary man a more and more real position. It is generally admitted, that, about the close of the glacial period, savage man killed the huge maned elephants, or fled from the great lions and tigers on what was then forest-clad valley-bottom, in ages before the later waterflow had cut out the present wide valleys fifty or a hundred feet or more lower, leaving the remains of the ancient drift-beds exposed high on what are now the slopes. The evidence of caverns such as those of Devonshire and Perigord, with their revelations of early European life and art, has been supplemented by many new explorations, without shaking the conclusion arrived at as to the age known as the reindeer period of the northern half of Europe, when the mammoth and cave bear and their contemporary mammals had not yet disappeared, but the close of the glacial period was merging into the times when, in England and France, savages hunted the reindeer for food, as the arctic tribes of America do still. The evidence increases as to the wide range of paleolithic man. He extended far into Asia, where his characteristic rude stone implements are plentifully found in the caves of Syria and the foot-hills of Madras. The question with which this section may have especial means of dealing is, whether man likewise inhabited America with the great extinct animals of the quaternary period, if not even earlier, — a question which leads at once into the interesting argument, how far any existing people are the descendants and representatives of man of the post-glacial period. The problem, whether the present Eskimos are such a remnant of an early race, is one which Professor Boyd Dawkins has long worked at. Since he stated this view in his work on cave-hunting, it has continually been cited, whether by way of affirmation or denial, but always with that gain to the subject which arises from a theory based on distinct facts. To be mentioned as preliminary are the questions, Were the natives met with by the Scandinavian seafarers of the eleventh century Eskimos? and, Whereabouts on the coast were they actually found? When the race of bold sea-rovers who ruled Normandy, and invaded England, turned their prows into the northern and western sea, they passed from Iceland to yet more inclement Greenland; and thence, according to Icelandic records, which are too consistent to be refused belief as to main facts,

they sailed some way down the American coast. But where are we to look for the most southerly points which the sagas mention as reached in Vineland? Rafn confidently maps out these places about the promontory of Cape Cod, in Massachusetts; and this has been repeated since, from book to book. Mr. Tylor pleaded guilty to having cited Rafn's map, but now felt bound to say that the voyages of the Northmen ought to be reduced to more moderate limits. It appears that they crossed from Greenland to Labrador (Helluland), and thence, sailing more or less south and west, in two stretches of two days each, they came to a place near where wild grapes grew, whence they called the country Vine-land. This would therefore seem to have been somewhere about the Gulf of St. Lawrence; and it would be an interesting object for a yachting-cruise to try, down from the east coast of Labrador, a fair four-days' sail of a viking ship, and identify, if possible, the sound between the island and the ness, the river running out of the lake into the sea, the long stretches of sand, and the other local features mentioned in the sagas, thus throwing light on the southern limit of the Eskimos. The *skrálings*, who came on the sea in skin canoes (*húdhkeipr*), and hurled their spears with slings (*valslöngva*), seem by these very facts to have been probably Eskimos; and the mention of their being swarthy, with great eyes and broad cheeks, agrees tolerably with this. If we may take it that Eskimos eight hundred years ago, before they had ever found their way to Greenland, were hunting seals on the coast of Newfoundland, and caribou in the forest, their life need not have been very unlike what it is now in their arctic home. Some day, perhaps, the St. Lawrence and Newfoundland shores will be searched for relics of Eskimo life, as has been done with such success in the Aleutian Islands by Mr. W. H. Dall; though on this side of the continent we can hardly expect to find, as he does, traces of long residence, and rise from a still lower condition.

Surveying, now, the vast series of so-called native or indigenous tribes of North and South America, we may admit that the fundamental notion on which American anthropology has to be treated is its relation to Asiatic. This kind of research is, as we know, quite old; but the recent advances of zoölogy and geology have given it new breadth, as well as facility. The theories which account for the widely lying American tribes, disconnected by language as they are, as all descended from ancestors who came by sea in boats, or across Bering Strait on the ice, may be felt somewhat to strain the probabilities of migration, and are likely to be remodelled under the information now supplied by geology as to the distribution of animals. It has become a familiar fact, that the Equidae, or horse-like animals, belong even more remarkably to the new than to the old world. There was plainly land-connection between America and Asia, for the horses whose remains are fossil in America to have been genetically connected with the horses re-introduced from Europe. To realize this ancient land-connection of Asia and America, — this 'tertiary-bridge,' to use Professor Marsh's expres-

¹ Abstract of an address to the section of anthropology of the British association at Montreal, Aug. 28, 1884, by EDWARD B. TYLOR, D.C.L., F.R.S., president of the section.

sion, — it is instructive to look at Mr. Wallace's chart of the present soundings, observing that an elevation of under two hundred feet would make Bering Strait land, while moderately shallow sea extends southward to about the line of the Aleutian Islands, below which comes the plunge into the ocean depths. If, then, we are to consider America as having received its human population by ordinary migration of successive tribes along this highway, the importance is obvious of deciding how old man is in America, and how long the continent remained united with Asia, as well as how these two difficult questions are bound up together in their bearing on anthropology.

To clear the obscurity of race-problems, as viewed from the anatomical stand-point, we naturally seek the help of language. Of late years the anthropology of the old world has had ever-increasing help from comparative philology. Within America the philologist uses with success the strong method of combining dictionary and grammar in order to define his great language-groups; such as the Algonquin, extending from Hudson's Bay to Virginia, the Athapascan, from Hudson's Bay to New Mexico, both crossing Canada in their vast range. But attempts to trace analogies between lists of words in Asiatic and American languages, though they may have shown some similarities deserving further inquiry, have hardly proved an amount of correspondence beyond what chance coincidence would be capable of producing. Thus when it comes to judging of affinities between the great American language-families, or of any of them, with the Asiatic, there is only the weaker method of structure to fall back on. Here the Eskimo analogy seems to be with North Asiatic languages, presenting in an exaggerated form the characteristic structure of the vast Ural-Altaic or Turanian group of Asiatic languages.

The comparison of peoples according to their social framework of family and tribe has been assuming more and more importance since it was brought forward by Bachofen, McLennan, and Morgan. One of its broadest distinctions comes into view within the Dominion of Canada. The Eskimos are patriarchal, the father being head of the family, and descent and inheritance following the male line. But the Indian tribes farther south are largely matriarchal, reckoning descent, not on the father's, but the mother's side. In fact, it was through becoming an adopted Iroquois that Morgan became aware of this system, so foreign to European ideas, and which he supposed at first to be an isolated peculiarity. No less a person than Herodotus had fallen into the same mistake over two thousand years ago, when he thought the Lykians, in taking their names from their mothers, were unlike all other men. It is now, however, an accepted matter of anthropology, that, in Herodotus's time, nations of the civilized world had passed through this matriarchal stage, as appears from the survivals of it retained in the midst of their newer patriarchal institutions. For instance: among the Arabs to this day, strongly patriarchal as their society is in most respects, there survives that most matriarchal idea that

one's nearest relative is not one's father, but one's maternal uncle. He is bound to his sister's children by a 'closer and holier tie' than paternity, as Tacitus says of the same conception among the ancient Germans. Obviously, great interest attaches to any accounts of existing tribes which preserve for us the explanation of such social phenomena. Some of the most instructive of these are too new to have yet found their way into our treatises on early institutions: they are accounts lately published by Dutch officials among the non-Islamized clans of Sumatra and Java. Among the Malays of the Padang Highlands of Mid-Sumatra, who are known to represent an early Malay population, not only kinship, but habitation, follows absolutely the female line; so that the numerous dwellers in one great house are all connected by descent from one mother, one generation above another, children, then mothers and maternal uncles and aunts, then grandmothers and maternal great-uncles and great-aunts, etc. There are in each district several *suku*, or mother-clans, between persons born, in which marriage is forbidden. Here, then, appear the two well-known rules of female descent and exogamy; but now we come into view of the remarkable state of society, that, though marriage exists, it does not form the household. The woman remains in the maternal house she was born in, and the man remains in his. His position is that of an authorized visitor; if he will, he may come over and help her in the rice-field, but he need not: over the children he has no control whatever; and, were he to presume to order or chastise them, their natural guardian, the mother's brother (*mamak*), would resent it as an affront. The law of female descent, and its connected rules, have as yet been mostly studied among the native Americans and Australians, where they have evidently undergone much modification. Thus, one hundred and fifty years ago, Father Lafitau mentions that the husband and wife, while, in fact, moving into one another's hut, or setting up a new one, still kept up the matriarchal idea by the fiction that neither he nor she quitted their own maternal house. But, in the Sumatra district just referred to, the matriarchal system may still be seen in actual existence, in a most extreme and probably early form. If, led by such new evidence, we look at the map of the world from this point of view, there discloses itself a remarkable fact of social geography. It is seen that matriarchal exogamous society (that is, society with female descent, and prohibition of marriage within the clan) does not crop up here and there, as if it were an isolated invention, but characterizes a whole vast region of the world. If the Malay district be taken as a centre, the system of intermarrying mother-clans may be followed westward into Asia, among the Garos, and other hill tribes of India. Eastward from the Indian Archipelago it pervades the Melanesian islands, with remains in Polynesia; it prevails widely in Australia, and stretches north and south in the Americas. This immense district represents an area of lower culture, where matriarchalism has only in places yielded to the patriarchal system, which develops with the idea of property, and which, in the

other and more civilized half of the globe, has carried all before it, only showing in isolated spots, and by relics of custom, the former existence of matriarchal society. Such a geographical view of the matriarchal region makes intelligible, facts which, while not thus seen together, were most puzzling. Though it is only of late that this problem of ancient society has received the attention it deserves, it is but fair to mention that its scientific study began long ago, in the part of the world where we are assembled. It is remarkable to find Father Lafitau already pointing out, in 1724, how the idea of the husband being an intruder in his wife's house bears on the pretence of surreptitiousness in marriage among the Spartans. He even rationally interprets in this way a custom which to us seems fantastic, but which is a most serious observance among rude tribes widely spread over the world. A usual form of this custom is, that the husband and his parents-in-law, especially his mother-in-law, consider it shameful to speak to or look at one another, hiding themselves, or getting out of the way, at least in pretence, if they meet. The comic absurdity of these scenes, such as Tanner describes among the Assineboins, disappears if they are to be understood as a legal ceremony, implying that the husband has nothing to do with his wife's family.

It is obvious that in this speculation, as in other problems now presenting themselves in anthropology, the question of the antiquity of man lies at the basis. Of late, no great progress has been made toward fixing a scale of calculation of the human period; but the arguments as to time required for alterations in valley-levels, changes of fauna, evolution of races, languages, and culture, seem to converge more conclusively than ever toward a human period, short, indeed, as a fraction of geological time, but long as compared with historical or chronological time. While, however, it is felt that length of time need not debar the anthropologist from hypotheses of development and migration, there is more caution as to assumptions of millions of years where no arithmetical basis exists, and less tendency to treat every thing prehistoric as necessarily of extreme antiquity; such as, for instance, the Swiss lake-dwellings and the Central-American temples. There are certain problems of American anthropology which are not the less interesting for involving no considerations of high antiquity: indeed, they have the advantage of being within the check of history, though not themselves belonging to it.

A brief account may now be given of the present state of information as to movements of civilization within the double continent of America. Conspicuous among these is what may be called the northward drift of civilization, which comes well into view in the evidence of botanists as to cultivated plants. To see how closely the two continents are connected in civilization, one need only look at the distribution on both of maize, tobacco, and cocoa. It is admitted as probable, that, from the Mexican and Central-American region, agriculture travelled northward, and became established among the native tribes. This direction may be clearly traced in a sketch of

their agriculture. The same staple cultivation passed on from place to place. Agriculture, among the Indians of the great lakes, is plainly seen to have been an imported craft by the way in which it had spread to some tribes, but not to others. The distribution of the potter's art is similarly partial. With this northward drift of civilization other facts harmonize. Now that the idea of the mound-builders being a separate race of high antiquity has died out, and their earth-works, with the implements and ornaments found among them, are brought into comparison with those of other tribes of the country, they have settled into representatives of one of the most notable stages of the northward drift of culture among the indigenes of America.

NOTES AND NEWS.

IN order to facilitate the work of the Electrical conference to be held in Philadelphia, the chief signal-officer has issued to the members of the conference the following subjects, as suggested for discussion, with a view to recommending proper observations and reports: 1. What unpublished records exist in the hands of electric-lighting, telegraph, and telephone companies, relative to ground-currents and atmospheric or auroral influences? 2. What is the general experience on east-west, north-south, and other lines? 3. What records can be kept by managers of all lines without interfering with daily business? 4. What special observations can be made? 5. What special lines can be, perhaps, wholly devoted to the continuous record of the phenomena? 6. Do, or can, the noises and currents, as observed on telephone and telegraph lines, give information as to the location and future movement of a thunder-storm, aurora, rain, cold wave, etc.? 7. Are observations on buried lines, or those covered with metallic tubing, or double aerial lines, specially desirable? 8. How can we best secure a complete daily electric survey of a given small portion of country, and a general survey of a larger region? 9. What is practicable in the way of securing a daily map of the distribution of atmospheric and terrestrial electric potentials? 10. Who will maintain self-recording electrometers?



FIG. 1.



FIG. 1a.

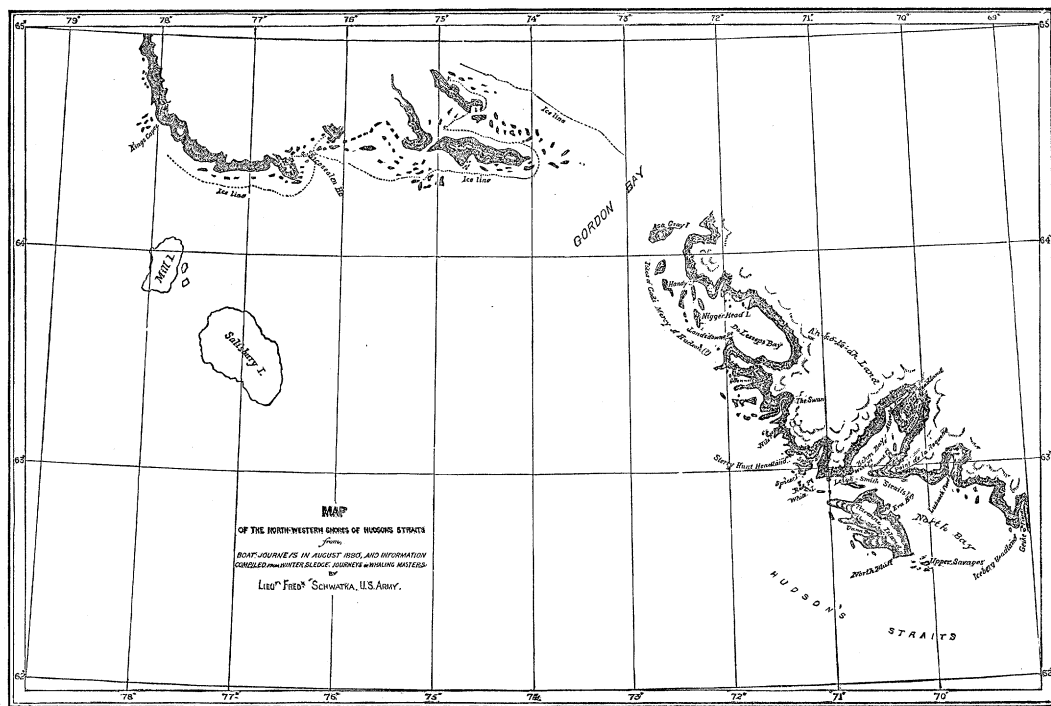
FIG. 2.

— The ability of flies to walk on glass and other polished surfaces receives a new explanation at the hands of Dr. J. E. Rombouts in the *Archives du*

musée Teyler, ser. ii. part 4. He denies the former views, that it is due to pressure of the air, or to the effect of a viscous liquid exuded by the foot, and says that it can be accounted for only by capillary action. In order to study the process, he enclosed a fly in a thin box with a glass plate as a bottom; and, when the box was turned so that the glass was uppermost, the feet could easily be studied under the microscope. The cushions of the fly's feet are distinctly seen to be covered with club-shaped hairs (fig. 1., 1a.) to the number of eight hundred or one thousand, arranged with considerable regularity. From these a fatty liquid is exuded, which leaves on the glass a trace of their contact (fig. 2). The ability to

from whalers, and gives the position of many localities first named by him.

— Dr. Chervin has been studying the medical geography of the department of the Seine inférieure with reference to disabilities which are developed by the annual conscription. The period chosen covered twenty years. After those cases of feeble constitution, evidently unfit for military duty, the most frequent disability was dental caries, after which followed hernia, etc. In considering the department as a whole, the average number of conscripts rejected on this account was fifteen per cent; but, in considering the separate cantons, it was shown that they vary greatly in this regard, the least average



adhere to the glass arises from the attraction exercised by each of these little drops of liquid on the hair from which it is exuded. Various experiments with hairs are recorded to show that capillary force would be sufficient to easily bear the weight of a fly, even were the fluid pure water.

— The accompanying map of the north-western shores of Hudson's Strait is of interest at the present time, on account of the expedition which lately went there under the command of Lieut. A. R. Gordon, to gather information as to the possibility of using the strait in a line of water-connection from the west to Europe. The map was compiled by Lieut. Frederick Schwatka, from surveys made by him while on boat-journeys in August, 1880, and winter sledge-journeys, and from information gathered

being eight per cent, and the highest nearly twenty-four per cent. In seeking a cause for this singular difference, that sometimes alluded to, the drinking of sour Norman cider, was considered to have little real influence. The question of race was believed to be more important. So far as form and height are concerned, the tables for twenty years showed two physical groups or races,—the smaller in the west; the larger in the east, especially about Dieppe and Neufchatel. The taller race is much more subject to dental caries than the other, and this is confirmed by testimony from other departments. Baldness prevails to such an extent, that two per cent of the recruits examined were exempt, on that account, at the early age of twenty. Some connection appears to exist between this deficiency and decay of the teeth.